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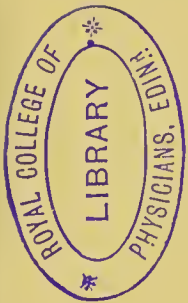
CONTRIBUTIONS TO PRACTICAL MEDICINE.

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CONTRIBUTIONS

TO

PRACTICAL MEDICINE.



BY

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Birmingham.

CORNISH BROTHERS.

1886.

Alexander Fleming:

In Memoriam.

PREFACE.

From such of my medical writings as have been published previously, as clinical lectures, essays, and annotations, in various professional periodicals, during the last eighteen years, I have been induced to select some which I hope may not be thought unworthy of reproduction. These I have rewritten, pruned, and amplified, corrected by my later experience, and collected in this volume.

22, TEMPLE ROW,
BIRMINGHAM, 1886.

CONTENTS.

	PAGE
<p>I. THE CAUSES AND CURE OF INSOMNIA. (A clinical lecture, published in <i>The Lancet</i>, June 15th and 22nd, 1878.)</p> <p>Psychic, toxic, and senile forms of insomnia. —Condition of brain in sleep.—Emotional shock and mental strain as causes of insomnia.—Symptoms of insomnia.—Causes of toxic insomnia.—Treatment ...</p>	1—21
<p>II. PHTHISICAL LARYNGITIS. (A clinical lecture, published in <i>The Lancet</i>, January 30th, 1875.)</p> <p>Definition of the disease.—Value of the laryngoscope.—Concurrence of phthisical laryngitis and pulmonary consumption.—Stages of phthisical laryngitis.—Vocal symptoms.—Respiratory symptoms.—Difficulty of deglutition.—Cough.—Pain.—Expectoration</p>	22—30
<p>III. THE TREATMENT OF PHTHISICAL LARYNGITIS. (A paper published in <i>The Birmingham Medical Review</i>, February, 1883.)</p> <p>General treatment.—Counter-irritation.—Laryngeal brushings.—Inhalations.—Lozenges.—Rest</p>	31—37

- IV. NOTE UPON THE MEDICINAL TREATMENT OF PULMONARY CONSUMPTION, WITH ESPECIAL REFERENCE TO THE VALUE OF CHLORIDE OF CALCIUM. (Abstract of a paper published in *The British Medical Journal*, June 5th, 1880.)
 Phthisis a generic name. — Various remedies required.—Value of chloride of calcium as a general remedy in phthisis.—Its old repute in struma.—Value in night-sweats 38—43
- V. SOME POINTS IN THE TREATMENT OF THE SEVERER FORMS OF CONSTIPATION AND OF INTESTINAL OBSTRUCTION. (A paper published in *The British Medical Journal*, November 17th, 1883.)
 Difference between Constipation and Intestinal Obstruction. — Remedies in habitual constipation. — Signs of fæcal retention.—Enemata.—Varieties of intestinal occlusion.—Symptoms of intestinal occlusion.—Treatment of intestinal occlusion.—Value of combinations of therapeutic resources.—When surgical interference is indicated.—Choice of surgical procedure... 44—72
- VI. ACCENTUATION OF THE PULMONARY SECOND SOUND OF THE HEART. (A clinical lecture, published in *The British Medical Journal*, March 31st, 1883.)
 What accentuation indicates. — Clinical import.—Prognostic value.—Therapeutic indications 73—83

PAGE

- VII. REMARKS ON FLOATING KIDNEY. (A digest of two papers: Floating Kidney, *The Birmingham Medical Review*, July, 1872; Remarks on Floating Kidney, *Ibid.*, October, 1883.)

Cases of floating kidney established by post-mortem examinations. — Physical signs. — Case. — Causation of floating kidney. — Frequency in women. — Comparison of anatomical relations of kidneys. — Frequency of floating kidney in women. — Symptoms.—Treatment... ..

84—95

- VIII. THERAPEUTIC PROGRESS. (A Presidential Address, published in *The Medical Times and Gazette*, August 8th, 1885.)

Comparative backwardness of therapeutics. — Difficulty of therapeutic inference. — Therapeutic triumphs.—Neglect of therapeutic teaching.—Danger of false theories. — Right relations of science and practice. — Recent therapeutic progress.—Scope of therapeutics.—Conditions of therapeutic progress.—Physiological research in therapeutics. — We must yet be empirics. — Progress proceeds by the discovery of the unknown, and by the perfection of the known

96—117

I.

THE CAUSES AND CURE OF INSOMNIA.*

Psychic, toxic, and senile forms of insomnia.—Condition of brain in sleep.—Emotional shock and mental strain as causes of insomnia.—Symptoms of insomnia.—Causes of toxic insomnia.—Treatment.

I desire to submit to you some practical observations, based upon my own experience as a physician, concerning the etiology and management of insomnia. Inability to sleep at all, or at a suitable time, or long enough, without the aid of drugs, is one of the commonest complications and consequences of a vast variety of morbid states. Pyrexia; physical pain, if sufficiently severe, and from whatever cause arising; frequent coughing, as that which often occurs in chronic pulmonary phthisis; urgent dyspnoea, such as results from extreme dilatation of the cardiac cavities, and requires an extraordinary vigilance of the nervous centres for the maintenance of the

* A Clinical Lecture: published in *The Lancet*, June 15th and 22nd, 1878; since revised and entirely re-written.

processes of respiration and circulation — are conditions which prevent, shorten, or interrupt sleep. In such and similar instances the cause of the sleeplessness is obvious. Such insomnia may mostly be controlled by one of two therapeutic methods, or by a combination of them, namely—either by the exhibition of remedies which directly promote sleep (hypnotics), or by the adoption of measures which combat the cause of the insomnia, by reducing the fever, by removing the pain, by soothing the cough, or by relieving the cardiac disturbance. But of sleeplessness arising as the direct effect of these and like conditions it is not my purpose to speak. I shall endeavour to unravel the complex causes, and point out the successful treatment of that kind of insomnia which may be called, for the sake of simplicity, but scarcely with strict truth, *insomnia per se*, or simple inability to sleep—a kind of wakefulness for which we fail to find an objective or obvious physical cause, and which seems to depend upon an inability on the part of the brain and nervous system generally to adapt themselves to the conditions which are necessary for sleep. We meet with this disorder more in private than in hospital practice—mostly in persons who belong to what is called the upper middle class, and mostly in individuals of high mental endowment. The malady is of extreme importance, and happily, if its causes be rightly

understood and judiciously controlled, there are few affections which are more within the sphere of curative therapeutics.

A close study of cases of the kind of insomnia of which I am speaking reveals striking differences in individual instances of the disorder, in respect both to the causes and the course of the malady. These differences demand careful consideration, because they have important bearings upon both our therapeutics and our prognosis. I have endeavoured to arrange the different varieties of insomnia into groups, in which the cause of the affection is the "principle of division." To these groups I give the names "psychic," "toxic," and "senile."

In natural sleep the brain is relatively anæmic. When the organ is in full activity its arteries are filled with blood; its cells are living rapidly, actively receiving nourishment from the blood, and pouring into it in exchange the effete products of their vitality. But in sound sleep the brain is inactive; at least all but that part of it which is concerned in the processes of organic life. The cells which think are as still as those of the muscles of a limb which is at rest. The blood flows in a smaller and gentler stream than in the waking state; the cells are not expending energy, but they are leisurely renewing it and storing it up: in a word, they are resting. Any cause

which prevents the due repose of a sufficient number of those brain-cells which are concerned in conscious thought will prevent sleep; relative cerebral hyperæmia is a consequence of such activity, and is also a concurrent, though subordinate, cause of wakefulness. Hence there are causes of insomnia which act primarily in sustaining cerebral activity, and with it, and in consequence of it, relative cerebral hyperæmia. Again, any cause which prevents the brain from becoming sufficiently relatively anæmic for sleep will produce insomnia. Any agent which sustains cerebral hyperæmia, or any morbid condition which impairs the contractility of the cerebral arteries, may prevent wholly, or in part, the occurrence of such a degree and extent of cerebral anæmia as is required for the production of sleep. Hence there are causes of insomnia which act primarily in exciting and sustaining relative cerebral hyperæmia, and with it, and in consequence of it, cerebral activity. But in such a complex condition as conscious cerebral activity, where thought implies increased blood-flow, and increased blood-flow implies thought, we cannot in any given case allow, with strict accuracy, entire causal precedence to either of the factors which are essential to the common result. But these considerations are strictly pertinent to a clear conception of the causes of insomnia. In

some cases of sleeplessness, as in the psychic group, undue and protracted cerebral activity is the primary vice; in others, as in the toxic and senile varieties, relative cerebral hyperæmia is the initial error, and cerebral action its direct consequence.

I shall now consider more in detail the psychic form of insomnia. A severe and sudden emotional shock of a depressing kind, as grief at the death of a beloved relative, will sometimes produce at once persistent insomnia, which will only yield to carefully directed therapeutic procedures. Prolonged mental strain, in all its varied phases, is a common cause of sleeplessness. Our patient may be a student preparing for an examination. For weeks, in spite of fatigue, he may have shortened his hours for sleep that he might lengthen his time for reading; and he may have been in the habit of keeping himself awake, when he could readily have fallen asleep, by drinking strong tea or coffee or by smoking tobacco. But he could always go to sleep at once when he went to bed, and sleep soundly, until, after some weeks of his abnormal work, with the nearer approach of the examination bringing increased anxiety as to the result of the ordeal, he found he began to sleep badly or could not sleep at all. He grew miserable; he could not remember what he read; he felt unfit for any exertion; and he could not

face his examination. Or, our patient may be a young professional man. He has commenced practice, or rather to wait for practice, as a barrister, a solicitor, a surgeon, or a physician. He begins to find that causes or cases have not been waiting for his advent; clients or patients are "few and far between." For a time he manfully struggles on, his hope and his health sustaining him; but these at last yield under the continued pressure of new disappointments and accumulating anxieties. He wants money; his friends will give it to him readily if he will ask for it, but his pride prevents him. It is not a gift or loan he needs; he does not want to beg or borrow money, he yearns to earn it. And while he has been hoping and waiting, and growing sick with the failure of his expectations, he has been working early and late in his study, perhaps reading for some higher examination, denying himself due sleep and exercise, in the trust that he might thus so skill himself as to secure the longed-for practice. At last he has fairly broken down. He has grown thinner; he looks haggard; he is filled with groundless fears; he is weighed down with the ineffable misery of insomnia; he has constant headache and noises in his ears; he thinks his memory is failing; he is dull and listless; he has been lying awake for hours after going to bed, or, waking in the "small hours," he has been unable

to sleep again, and when he has slept he has had horrid dreams; and he comes to us for help because he can scarcely sleep at all, and he is possessed by the fear that he is going mad. In these cases acute or continued mental strain is the primary cause of the sleeplessness. Where the shock has been sudden and severe, it has been sufficient to rouse a given group of cells into persistent activity. Where the strain has been less intense, but kept up long, it has been all the more hurtful because the same set of ideas has been maintained in exhausting recurrence; and because, as a consequence of this monotony, the same part of the brain has been continuously upon the rack. But in either case sleeplessness did not occur until there arose from exhaustion partial or complete vaso-motor paralysis of the intra-cranial blood-vessels; until the arteries of the brain, worn out by a sustained erethism, could no longer, even when the brain most needed it, find the force for that contraction of their calibre without which sleep is impossible.

The subjects of the psychic form of insomnia are mostly men, and mostly men of the nervous temperament. We have lately been too ready to ignore temperaments; our fathers studied them better and regarded them more than we do. But I shall not go to any authority for a portrait of the nervous temperament; I shall describe it as I

think I have found it. I use the phrase nervous temperament to indicate a distinct type of outward form, of manner, of habits, and of tendencies. Temperaments present their strongest types most commonly in men ; comparatively few women exhibit a well-marked temperament. Two or more of the different kinds of temperament may appear to be blended ; we have a compound of modified temperaments. A man of distinctly nervous temperament has a quick manner ; he is nearly always in a hurry ; he is apt to talk volubly and eat quickly ; if he does not know us well, he fidgets with his hands or legs when he is speaking ; he talks abruptly, earnestly, and fluently, often splitting up his phrases, or recalling and correcting them, and especially modifying qualifying words, such as adverbs and adjectives, in his anxious desire to express what he conceives to be the finest shades of truth. When he becomes a patient he is harassed about some trivial symptom ; he has felt his heart beating, and he thereupon fancies he has some deadly cardiac disease ; he thinks his memory is failing, and he forthwith imagines he is going mad.

A man who has suffered much from insomnia becomes the subject of a well-marked group of symptoms. Most of them are given by certain writers amongst the signs of cerebral hyperæmia. It is probable that they mark a particular variety of

exhaustion of the brain, attended by more or less abnormal increase of intra-cranial vascularity, and accompanied by some general prostration of the bodily powers. Here are the concomitants of insomnia as I have found them. The patient has a dull and listless look ; his eyes are wanting in vivacity ; the upper lids may droop a little, and they may be slightly swollen. The complexion is sallow. There is headache ; of this there are two kinds, which either co-exist or occur separately. The commoner variety of headache is a dull pain felt over the whole of the vertex, together with a vague and widespread feeling of oppression in the head ; the other is a sharp, shooting pain, which comes on suddenly, and usually in single flashes, and which gives the idea of a knife being driven through the head from one temple to the other. Occasionally the patient feels a momentary giddiness ; this may cause a false step, but it never lasts long enough to give rise to staggering. The skin of the head, especially near the saggital suture, may be tender. There are noises in the ears, in one or in both, usually of a low-pitched whistling character. They may come on suddenly, and without apparent cause, as when the patient is talking quietly, or they may only come on when the attention is more closely occupied, as in writing a letter or casting-up figures. A striking sign is a slight impairment of hearing. The patient may

be unaware of it, but those with whom he lives have noticed that he often asks them to repeat what they say to him because he could not quite catch their words. He may complain of seeing spots before his eyes—little cobwebby, black lines, which come and go and float about, or bright, bluish, phosphorescent-like specks which seem fixed for a moment, one before each eye, and which only appear when he first directs his eyes towards an object. There are some abnormal sensations in the skin; not formication, such as is apt to arise in organic nervous disease, but a sharp, transitory, and isolated prickling, as of the movement of a single pin, which lasts only for an instant, and affects either the limbs or the trunk, mostly the former. There may be a peculiar twitching of muscles. It is not a vibratory tremor, like that of progressive muscular atrophy, nor is it a contraction of a whole muscle, or of a group of muscles, such as arises in true convulsion. But, while the patient is sitting still, a considerable part of a muscle becomes the subject of rapid clonic movements, and these are wholly independent of volition. These movements mostly occur in the lower extremities, but they are rarely sufficient to move the limbs; they usually affect the lower part of one vastus internus, and last for about a minute. The patient can feel the movements by attending to the affected part, and

he can also feel that the muscle moves by applying his hand to it. In such a case there is often unnatural and painful sensitiveness to external impressions. The patient craves for quiet. A bright light troubles him. Noises, the sight of moving objects, touches, as of the hand of a friend upon his shoulder, annoy him. There is not an increased sensitiveness to external impressions, but impressions which are enjoyed or unnoticed in health become irritants.

I have been considering a kind of insomnia in which unnatural excitation of the cerebral cells is probably the initial fault. In a very few words I shall indicate the other forms of the disorder—the toxic and the senile.

In toxic insomnia, the cause of the sleeplessness acts primarily upon the vessels of the brain, giving rise to some degree of arterial hyperæmia. Some poisonous agent maintains cerebral vascularity at such a height that conscious cerebral activity—that is, wakefulness—is an inevitable consequence. Such a poison may be introduced into the body from without, or it may be a product of diseased processes arising within the body itself. Of course, I use the word “poison” in a restricted sense; I do not mean something which kills, but only something which produces abnormal manifestations in the living body. The external poisons which most frequently cause sleeplessness

are tobacco, alcohol, tea, and coffee; the internal, certain effete products of tissue-metamorphosis which accumulate in the bodies of gouty persons, or of those whose kidneys act deficiently. Many a man does not and cannot sleep sufficiently simply because he smokes excessively. Cut off his "cavendish" or his cigars, and he will sleep well. Many smokers know that they sleep badly if they smoke more than their usual quantity of tobacco, or if they smoke tobacco stronger than that to which they are accustomed. If a man who smokes two cigars every evening is induced at some time to smoke three, or if a smoker of "bird's-eye" ventures to replace it by "cavendish," he may, when he has gone to bed, find he cannot sleep; and the cause of his sleeplessness is the smoking of more or of stronger tobacco than that to which he has been accustomed. Men of distinctly nervous temperament, or men in whose temperament there is a distinct and considerable admixture of the nervous element, are often great smokers. Men who are slow and calculating are rarely smokers; men whose activity is of an objective type are happy in rarely feeling the nervous unrest which tobacco calms. Tobacco smoking stimulates the cerebral circulation; it disposes to a succession of pleasing ideas by inducing an easy flow of mental activity. But this stimulation of the blood-flow in the brain is sure,

if pushed to undue limits, to induce cerebral vasomotor debility or paralysis, and, as a consequence, persistent conscious thought. Sometimes, then, a man consults us for the relief of insomnia, and we find he is young, he has had no trouble, he takes plenty of food and exercise, and he does not overtax his brain. But he is an excessive smoker; he smokes morning, noon, and night, and he has gone on from mild tobacco to the strongest. He need not give up, or shorten, or change his work, and he does not need drugs; cut off or cut down his smoking, and he at once sleeps well. And so, *mutatis mutandis*, does alcohol cause sleeplessness. The man who drinks to commencing drunkenness mostly sleeps soundly, if not well. But many a so-called moderate drinker knows that he sleeps badly if he takes a little more than his usual quantity of wine after dinner, or even his usual quantity of some unusual wine. Alcohol flushes and dilates the smaller blood-vessels, especially those of the brain; if such a condition be maintained, sleep is disturbed or wanting. We have all seen the insomnia of delirium tremens: the patient cannot sleep because the lesser arteries of his brain are paralysed by alcohol, and sleepless cerebral activity is the inevitable consequence. Far short of what is usually called alcoholism, we often meet with cases of insomnia in which alcohol alone is the cause of the wakefulness. The patient

may pride himself upon his moderate use of fermented stimulants, and he may be wholly ignorant of the cause of the sleeplessness for which he consults us. We fail to find any sufficient psychic cause for his insomnia; but if we take away or diminish his wine or his grog, or induce him to consume it before the evening, we find he soon begins to sleep well.

The effects of tea and coffee in causing wakefulness are well known. Some individuals are extremely susceptible to the action of these stimulants. We sometimes meet with persons, mostly women, who habitually drink enormous quantities of strong tea; such people are usually troubled with flatulent dyspepsia, and sleep badly, but they rarely suffer from serious insomnia.

On this occasion I can only mention those varieties of toxic insomnia which are apt to occur in gouty persons, or in those whose kidneys are failing, and which arise from the accumulation in the blood, in consequence of deficient excretion, of the products of tissue-metamorphosis. Insomnia of this kind is rarely complete. But the patient may complain that he sleeps very badly, that he lies awake for some hours and has great difficulty in getting off to sleep, that he is easily awakened and wakes frequently, and that he always dreams when he sleeps. In such a case we may find a pulse of high tension; the aortic second sound may be

accentuated, and the first sound of the heart may be reduplicated at the apex. Where there is chronic renal disease there may be the physical signs of the characteristic cardiac hypertrophy which accompanies chronic contracting nephritis. Insomnia in such cases is due, probably, to the maintenance of a state of high tension in the cerebral arteries. I wish to impress upon you that we find a clue to the cause of many cases of sleeplessness in the signs of the gouty diathesis or in the discovery of albuminuria.

Again, there is a senile form of insomnia. You may perhaps have observed amongst your friends that an exaggerated appreciation of the merits and value of early rising mostly increases as age advances. The sleeplessness from which many old persons suffer is mainly, if not entirely, the result of senile degeneration of the smaller cerebral arteries. Those vessels are less elastic and less contractile than in health, and their weakened walls often lead to their permanent dilatation; they are physically unable to adapt themselves fully to the condition of relative arterial anæmia which is requisite for healthy sleep. The tendency of this condition of the blood-vessels of the brain to prevent or lessen sleep is probably to a great extent counteracted by the cardiac feebleness which so frequently and so fortunately coexists with the vascular changes.

In the treatment of insomnia we must often use soporifics. Of these the chief are chloral, opium, morphia, the bromides, Indian hemp, alcohol, and affusion with cold water. The successful treatment of a case of sleeplessness follows from the discovery of its cause. In the severer forms of psychic insomnia we must often at once secure sleep by some efficient hypnotic. I prefer opium or chloral. By the use alone of one of these drugs we can often quickly cure acute insomnia depending upon some sudden mental shock or strain. A few nights of sound and sufficient sleep, artificially induced, will do more than anything else to restore to the brain the power of sleeping without aid from drugs. In the more chronic forms of psychic insomnia, where the sleeplessness or wakefulness usually depends upon prolonged worry or overwork, I employ chloral sparingly. It should only be used as a temporary remedy, when it is necessary that we should at once secure a fair amount of sleep. The patient ought never to be allowed to swallow this dangerous but valuable drug whenever he feels disposed, or to apportion its dose for himself; he ought only to take it upon the special prescription of his doctor. Another important point must always be kept in view. It is this: an overworked man must never be permitted to go on with his overwork, and habitually secure sleep by chloral

or any other hypnotic. In such a case we must always aim at preventing the sleeplessness by removing its cause, instead of pursuing the easier but illogical and precarious course of permitting that cause to continue, and trusting to counteract or suppress one of its effects by medicine. When a man cannot sleep because he works his brain too much, we must insist that he stop or greatly lessen his labour. But I must warn you that real work is not often the cause of insomnia. Work fits for rest. It is mostly worry, not work, that brings unrest. It is not work that wears, but worry. Whatever the cause of the insomnia, a holiday, with complete change of scene, will often do much to effect a cure. The old maxim, "*Cælum non animum mutant qui trans mare currunt*," like most other maxims, old and new, is not always wholly true. Send an overworked and worried merchant or barrister from his counting-house or from his chambers, in a busy town, to a quiet village by the sea, or across the Channel, to a French watering-place, and let him substitute walking and bathing, rowing and fishing, for his books or his briefs, and he will often need no physic to make him sleep soundly and sufficiently. But, however great the influence of new surroundings and of new outlets for energy, for the cure of many cases of psychic insomnia we cannot dispense with drugs. In well-nourished patients,

and in the slighter cases, bromide of potassium is by far the best hypnotic. It soothes the irritated and irritable cerebral cells; it is a direct and absolutely safe brain sedative. It is marvellously powerful in producing nervous calm; but it must be given in full doses, thirty to sixty grains at bedtime. It is well to conjoin with it some drug which will favour the contraction of the weakened cerebral vessels. For this purpose we may give tincture of ergot or tincture of digitalis, one or both. In many cases of chronic wakefulness, arising from mental strain, the patient is distinctly anæmic. Unless the anæmia be remedied, the insomnia cannot be cured. The patient's pale face and soft, small pulse declare the condition of his blood. Such a person mostly feels drowsy when he is up, and wakeful when he lies down. Of course he needs iron. We may give him a grain or two of reduced iron, sprinkled on a small piece of bread, or a wineglassful of Orezza water, after each meal. His diet must be liberal, containing plenty of fish, meat, and eggs. For such a patient alcohol is often the best hypnotic. To many people a "nightcap" of toddy is a superfluous, perhaps hurtful, luxury. But it gives, perhaps better than anything else, rest and sleep to the exsanguine and worried brain. We must never be blind to the responsibility we incur when we prescribe alcohol, neither need we exaggerate

that responsibility. When we use alcohol, in the form of any of the fluids which contain it, as a remedy in the treatment of disease, we must clearly state the reasons for its adoption; and we must discontinue it, as we discontinue the use of other drugs, when the conditions which called for its exhibition have disappeared. If I am sure of anything in therapeutics, I am sure that alcohol is the best hypnotic in many cases of chronic psychic insomnia, when the patient is worried, sorrowful, weakly, and anæmic.

Many comparatively minor points are especially worthy of attention in the treatment of chronic psychic insomnia. In most cases, whether he sleep well or ill, the patient ought, from day to day, to go to bed and get up at fixed and regular times. Healthy sleep is a rhythmic act; it tends to occur periodically. Daily bodily exercise, in the open air, if possible, but always short of great fatigue, must be enjoined. Riding in a carriage is good, walking better, riding on horseback best. A worn and self-worrying man, wrapt up in the absorbing torture of self-consciousness, may take exercise in a carriage or upon his legs and still keep up his fretting, but he must come out of himself when he gets into a saddle. Gardening affords good exercise, and it is very efficient in keeping up objective attention. Those who live in towns may find good objective

employment in chopping wood; if they have not trees to fell, they can at least copy Archbishop Whately, and give their minds a refreshing objective bent and their muscles healthy work by cutting up firewood. People who find it difficult to get off to sleep have been advised to count monotonously one, two, three, up to a thousand or more, until they fall asleep; to watch in imagination each sheep of a large flock squeezing through a narrow opening; to picture some familiar landscape and keep the mind fixed upon it; to repeat the letters of the alphabet, &c. These are expedients for changing the current of cerebration. For a night or two one or the other may succeed, but they cannot be relied upon. These practices often even keep up wakefulness; when the mind attends to them too closely, they sustain the subjectivity which keeps the brain from resting. Often the surest way of keeping awake is to try hard to go to sleep. We do most things best when we forget ourselves: going to sleep is no exception to the rule. Sleep may often be induced by the temporary application of cold to the head or to the general surface of the body. A person who has been lying awake will often fall asleep at once after getting out of bed and sousing his head, neck, and hands in cold water, or after following Charles Dickens' plan of standing at his bed side until he feels chilly,

and thereupon turning over, shaking up, and cooling his pillows and the bed-clothes, and then getting into bed.

In the toxic kinds of insomnia we must endeavour to act upon the maxim, "*cessante causâ, cessat et effectus.*" We must stop or lessen the consumption of tobacco, alcohol, tea, &c., as the case may be. A discussion of the treatment of gouty insomnia, and of the sleeplessness arising in some chronic kidney diseases, would involve a consideration of the whole question of therapeutics of the maladies upon which these forms of wakefulness depend. I shall only now say that in gouty lithiasis, with a pulse of high tension, I have confidence in the curative powers of colchicum, supplemented by the exhibition of dilute saline purgatives, such as Pullna, Friedrichshall, Hunyadi Janos, Æsculap, and Rakoczy waters. Senile insomnia is very obstinate; perhaps in the bromides, with full doses of hop or henbane, we have the most efficient and least harmful means for its relief.

II.

PHTHISICAL LARYNGITIS.*

Definition of the disease.—Value of the laryngoscope.

—Concurrence of phthisical laryngitis and pulmonary consumption.—Stages of phthisical laryngitis—Vocal symptoms.—Respiratory symptoms.—Difficulty of deglutition.—Cough.—Pain.—Expectoration.

I purpose to offer you a brief description of phthisical laryngitis. I shall endeavour to lay before you in outline, in as few words as possible, an account of the clinical history and progress of this common, painful, fatal, and important malady. By phthisical laryngitis, I mean that peculiar form of chronic inflammation of the structures of the larynx which is met with in a very large proportion of cases of ordinary chronic pulmonary consumption. From the earliest times this disorder has been recognised. Celsus indicated it, probably, when he described, as a result of exposure to bad weather, “Tussis, destillatio, raucitas, in quibusdam etiam tabes oritur.” The general symptoms of serious laryngeal disease are

* A Clinical Lecture: published in *The Lancet*, January 30th, 1875; since revised and entirely re-written.

always strikingly evident, even without the aid of the laryngoscope ; and when these were present in cases of consumption of the lungs, it was usual to infer that laryngeal phthisis existed as a complication. After death, decided evidences of grave laryngeal lesions were often found, more especially ulcerations of the mucous membrane and changes in the cartilages—conditions which have been carefully described by Rokitansky and other writers. But the laryngoscope, as has often been said, literally threw light upon these and many other pathological processes in the throat. We have now become able exactly to observe, and carefully to watch, and so intelligently to treat, these local maladies ; and it has become possible to mark the earliest visible alterations of the larynx in laryngeal phthisis, to recognise the disease in its first beginnings, to divide its progress into stages, and to apply remedies directly to the morbid tissues. Mackenzie, Niemeyer, Marcet, Cohen, Gibb, Aitken, Lennox Browne, and many others have written excellent descriptions of laryngeal phthisis—descriptions which agree in the main ; while they conflict in not a few important particulars. But I shall follow the writings of none of these authors ; I prefer to base my brief account of the course of the disease upon my own clinical experience in private and hospital practice.

Concerning the pathology of phthisical laryngitis I shall say little. Our ideas of the pathology of phthisis have recently undergone some important changes. Amid much contradiction, but as the fruit of much patient labour, very great advances—advances to my mind unsurpassed in importance by any which have been made in this generation in any department of the science of medicine—have been gained in our knowledge of phthisis and tuberculosis. Pathologists and physicians who have written about laryngeal phthisis have differed much in their views concerning the nature of the affection. For example, “Virchow recommends the larynx as the very place to study true tuberculosis;”^{*} while Louis did not consider that tubercle was ever deposited in the tissues of the larynx;[†] and Mackenzie, describing the pathology of laryngeal phthisis, says, “Tubercle appears to play a very secondary part, if any part at all.”[‡] My own observations lead me to conclude that the local lesions in the larynx in phthisis are inflammatory, and unconnected with truly tubercular processes. But we must recognise that the local changes in phthisical laryngitis have a characteristic course of their own; the local inflammation, in its progress and results, is distinctly

^{*} Niemeyer: Pract. Med., translated by Humphreys and Hackley.

[†] Louis on Phthisis, quoted in Reynolds' System of Medicine.

[‡] Mackenzie: Reynolds' System of Medicine.

of a specific type. Phthisical laryngitis is a peculiar form of chronic inflammation of the larynx.

I do not think I have ever met with a case of laryngeal phthisis which was not complicated, sooner or later, with pulmonary consumption. The pulmonary changes usually appear before those in the throat. But not unfrequently laryngeal phthisis becomes fully established before any lesions in the lungs can be appreciated, and in these cases the laryngoscope is of the greatest service in helping us to decide whether a given case of laryngitis, unattended by thoracic disease, is phthisical or not. Adults are much more liable to this disease than children, and men than women.

For some time past I have found it convenient to divide the laryngeal changes in this affection into four stages, viz., 1, the stage of anæmia; 2, that of tumefaction; 3, that of ulceration; and, 4, that in which necrosis or caries of the cartilages may arise. The first three of these stages are almost always met with; the last is not nearly so constant in its occurrence. The earliest visible local change is anæmia—a very decided and general pallor of the mucous membrane of the larynx. Any one accustomed to use the laryngoscope cannot fail at once to recognise this condition. Sometimes the anæmia extends to the whole of the upper part of the throat, to the fauces, the soft palate, and the roof

of the mouth; but it never goes beyond these limits; it is always local; in its localisation is its phthisical significance. When the signs of anæmia are general, this local bloodlessness loses its phthisical import; but when we find vocal feebleness and laryngeal anæmia in a person who is not anæmic, in the ordinary sense of that term, we must search for other evidences of phthisis, and closely watch and strive to improve the nutrition of the larynx. As the local disease progresses, the laryngeal mucous membrane becomes hyperæmic; the cords lose the clear, white lustre of health. For a time it may be very difficult or quite impossible to distinguish the case from one of simple chronic laryngitis. But sooner or later the characteristic tumefaction of laryngeal phthisis appears, and there remains no further room for doubting the serious nature of the local malady. The swelling is usually most marked in the ary-epiglottic folds. In extreme cases these folds are often expanded into two large, tense, brawny-looking, pear-shaped tumours, with their larger ends meeting together in the middle line behind, while their smaller extremities are directed upwards and outwards towards the epiglottis. The epiglottis is similarly changed; it loses its leaf-like form, its edges become thick and rounded, and the whole organ presents the appearance of an irregularly globular tumour. Sometimes the epiglottis

is more or less twisted and drawn to one side, and the swelling of the ary-epiglottic folds may be unilateral at first. These tumefied tissues seriously encroach upon the opening of the glottis, and they often completely hide the vocal cords from view. Even now the laryngeal mucous membrane looks paler than in health, although irregular patches of congestion and enlarged and tortuous vessels may frequently be observed. When the appearances of this stage of laryngeal phthisis have become established, the case is sure to terminate fatally. Sooner or later, if the patient do not die from disease in other organs, the swollen laryngeal tissues become ulcerated. The ulcers are irregular in shape and distribution; there are usually more than one of them, they may occur in any part of the larynx, they tend to spread and run together, and their surfaces are always covered with a mixture of pus and mucus. The ulceration frequently implicates the vocal cords, usually at the junction of their middle and posterior two-thirds. Caries or necrosis of some of the cartilages, proceeding probably from perichondritis, is the last local lesion in laryngeal phthisis, but this stage is rarely reached.

Let us regard the effects of these changes upon the voice, upon breathing, and upon swallowing. In all cases the voice is affected early; in nearly all it is sooner or later wholly extinguished. At

first the vocal impairment may be slight ; the voice appears weak and tends to become whispering. Failure of the voice in reading aloud may first be noticed ; those who sing find it difficult or impossible to produce their higher notes. When congestion appears, the voice becomes husky, harsh, and hoarse. When there is much submucous tumefaction, the movements of the cords, and especially their approximation, are mechanically interfered with, and the voice is consequently still further impaired ; when the tumefaction is considerable, and when it passes into widespread ulceration, all truly laryngeal voice is usually lost.

There is, as a rule, no embarrassment of respiration at first. But when the second stage of the malady is decidedly established, the passage of air through the narrowed glottis becomes more and more difficult ; there is often loud stridor, which in some cases is subject to alarming paroxysmal exacerbations, and in a few instances the laryngeal dyspnœa is so great that tracheotomy may be necessary to avert impending suffocation.

In most cases of laryngeal phthisis deglutition is at some time interfered with ; in many instances dysphagia becomes the most prominent and most dangerous of all symptoms. In many cases of laryngeal phthisis the symptoms of organic stricture of the œsophagus may be closely simulated. There

is never any difficulty in deglutition until the disease has at least reached its second stage. When the ary-epiglottic folds are largely swollen, they project backwards over the upper opening of the œsophagus, and offer a mechanical obstruction to the passage of food. In this stage, too, the swollen parts cannot be perfectly approximated so as to occlude the glottis and prevent the entrance of portions of food into the larynx during swallowing; almost whenever swallowing is attempted a violent fit of coughing is induced, during which fluid food may be ejected through the nostrils. Apart from these serious difficulties, deglutition always, sooner or later, becomes acutely painful. Pain arises in part from the mere pressure of passing food upon the tumid and tender laryngeal structures, but it is also due to the movements of the diseased tissues which the act of swallowing requires.

When the morbid parts are at rest, in many cases there is little local pain. There is nearly always a little unpleasant tickling in the throat as an early symptom. Patients feel as if there were something sticking in the windpipe—as a fly—and causing irritation, which they are prompted to try to remove by violent hawking and coughing. When the stage of ulceration is reached, severe burning pain in the throat is sometimes felt, which is propagated in various directions, but especially towards the ears.

A considerable quantity of phlegm may be derived from the larynx in phthisical laryngitis. In the earlier stages of the disease, nothing but a little glairy mucus may be coughed up; when there is much congestion, a few streaks of blood occasionally appear; when there is ulceration, the sputa gradually become chiefly purulent, and the admixture of blood is more copious and more frequent.

III.

THE TREATMENT OF PHTHISICAL LARYNGITIS.*

General treatment.—*Counter-irritation.*—*Laryngeal brushings,* *Inhalations.*—*Lozenges.*—*Rest.*

Although we cannot hope to cure laryngeal phthisis when it has become fairly established, that is when it has reached its second stage, we are able, nevertheless, to do a great deal of good in retarding the progress of the disease and in relieving its pains. By the second stage I mean the stage of laryngeal tumefaction, when the characteristic local swelling appears, replacing the previous anæmia, and leaving no further room for doubting the serious and fatal nature of the laryngeal affection. In all cases of phthisical laryngitis, and in all cases of chronic laryngitis, in which suspicious phthisical symptoms are presented, we ought to carry out, systematically and perseveringly, all the general medicinal and hygienic measures which are beneficial in the therapeusis of consumption. I do not regard

* A paper published in *The Birmingham Medical Review*, 1883, Vol. I.; since revised and entirely rewritten.

laryngeal phthisis as a local disease, any more than I look upon cancer as a local disease; it is a local expression of a general constitutional condition, albeit often determined by local causes. To this pathological doctrine I can add, from a fairly extensive experience, that I have never seen a well-marked case of laryngeal phthisis which was not complicated by distinct physical evidences of chronic consumption of the lungs. It is my present purpose only to direct attention to the special local treatment with which more general measures may be supplemented in the management of a case of phthisical laryngitis. Counter-irritation, in various forms and of various intensity; the application of remedies to the diseased parts by means of a laryngeal brush; inhalations, whether atomised, steam, or fuming; insufflations; and the local administration of medicines in the form of lozenge, are therapeutic methods which may be used in turn or in several combinations, to suit different practical indications. Counter-irritation over the front of the neck is likely to be of service at any time; it is especially useful when the larynx is congested or painful. We may use iodine or croton liniments, antimonial ointment, or mustard poultices or leaves. If the colour of iodine be objectionable, a useful liniment may be made by adding to a strong alcoholic solution of iodine enough liquor potassæ to discharge its

colour. A small fly-blister, placed just above the sternal notch, is sometimes of marked service. Stimulating and oleaginous liniments, well rubbed over the front of the neck, are useful; their employment seems to favour the expulsion of mucous accumulations from the diseased surfaces. The following is a good liniment of this kind:—

R. Olei Caryophylli, dr. ij.
 Liquoris Ammoniacæ, dr. iv.
 Olei Sambuci, ad un. ij.
 Misce, fiat linimentum.

Warm linseed or bread poultices, or pads of absorbent cotton wool, which wool is now sold variously medicated, wrung out of hot water, and covered with oiled silk, are soothing and grateful when laid over the front of the throat. My experience has led me to place considerable reliance on the efficacy of the application, by means of a suitable brush, of various solutions to the laryngeal mucous membrane. The brush must not be blindly plunged over the base of the tongue; it must be gently and accurately guided to the tumid and tender parts by the aid of a good light and a laryngeal mirror. Solutions of chloride of zinc, of sulphate of copper, or of nitrate of silver, of the strength of ten to twenty grains to an ounce of water, may be used. The solution of chloride of zinc has a stimulant, and that of sul-

phate of copper an astringent local action, while nitrate of silver combines these two effects. Local stimulants are especially indicated in the first stage of laryngeal phthisis, when we have to remedy the characteristic phthisical anæmia of the laryngeal mucous membrane. Astringents may be employed in the later stages, when there is swelling or ulceration. When deglutition is difficult, as it always is, sooner or later, in the progress of the affection, when the movement of the swollen parts is obstructed and painful, when the tense ary-epiglottic folds project backwards over the upper opening of the œsophagus, and when the aperture of the glottis cannot be properly protected during swallowing, prompt, though temporary, relief may be obtained by freely brushing the larynx with a strong solution of nitrate of silver, of the strength of one drachm to an ounce of distilled water. The application must be repeated every second or third day. We may also use many medicines as inhalations. These may be exhibited in various ways; for instance, as simple steam inhalations or as atomised inhalations, and in this latter class the spray may be produced either by steam, as in Siegle's apparatus, or by air driven by bellows, as in the well-known hand-ball spray-producers. As a general rule, moist and warm inhalations are best when we seek to soothe irritation, cold spray inhalations

when we desire an astringent or a stimulant effect, or both, while fuming inhalations are more suitable when we desire a stimulant and expulsive result, as in the removal of local collections of phlegm. The vapor coniæ of the British pharmacopœia is a good sedative. In the slighter cases, I often use with advantage the following spray inhalation :—

R. Aluminis, gr. 30.
Sodæ Biboratis, gr. 30.
Tr. Catechu, dr. iv.
Aq. Rosæ ad un. iv.
Misce, fiat solutio.

I can recommend the use of medicated lozenges in laryngeal phthisis, especially of some of those of the London Throat Hospital. Theoretically, or rather *a priori*, the local action of lozenges on the larynx may be doubted, but, if we would do our best as practitioners in our day and generation, we must accept empirical facts in therapeutics, and acquire the habit of not valuing remedies the less because we can neither explain nor understand their effects. I am sure good can be done by lozenges in laryngeal affections. In the earliest stages of laryngeal phthisis the trochiscus acidi benzoici, an excellent “voice lozenge,” is useful. Later, more emollient and sedative preparations must be employed, such as the lozenges of marsh-mallow, opium, morphia, or codeia.

Whether we use paints, sprays, powders, or lozenges, there comes a time in the progress of laryngeal phthisis when coughing is not only an urgent symptom to be treated, but also a condition to be arrested as destructive in its local tendencies, and as the prelude of vomiting, which may become so frequent as to break down rapidly the patient's remaining strength. Any measure or remedy, pharmacopœial or domestic, which can keep down coughing affords much relief and often substantial benefit in laryngeal consumption. Prompted by the feeling of local irritation, patients are prone to make energetic efforts at clearing the throat. Such efforts ought to be avoided as much as possible. It is astonishing how much the act of coughing can be brought under the control of the will by an intelligent and resolute person. We ought to impress upon our patients the importance of restraining and refraining from coughing so far as they are able to do so. While hawking and coughing may give a brief relief by removing accumulating secretions, such actions tend to perpetuate and increase laryngeal inflammatory changes. Here, as everywhere, rest is a prime factor in the control of inflammation. We ought to rest the larynx as much as possible. In the management of laryngeal phthisis keep steadily in view the fact that the malady is a chronic local and

specific inflammation. With this idea as the basis of therapeutic indications, you will have two associated lines of treatment, directing your remedies to the relief of local irritation, and to the reform of the constitutional vice of which the laryngeal lesion is a specific expression.

IV.

NOTE UPON THE MEDICINAL TREATMENT OF PULMONARY CONSUMPTION, WITH ESPECIAL REFERENCE TO THE VALUE OF CHLORIDE OF CALCIUM.*

Phthisis a generic name.—Various remedies required.
—Value of chloride of calcium as a general
remedy in phthisis.—Its old repute in struma.
—Value in night-sweats.

Have we a remedy for pulmonary consumption? I use the term pulmonary consumption in a generic sense, as including a variety of cognate pathological conditions, marked by certain common consequences and signs. We know that the term chronic pulmonary phthisis includes a variety of pathological conditions and a variety of textural

* Abstract of a paper read before the Birmingham and Midland Counties branch of the British Medical Association: published in *The British Medical Journal*, June 5th, 1880; since revised and entirely rewritten.

lesions in the lungs, which have long been recognised as distinct, which recent research has done much to unravel, and about which we may confidently expect to learn more. The differing, if cognate, clinical and pathological courses of tubercular phthisis, of unresolved lobar pneumonia, of chronic and catarrhal lobular pneumonia, and of pulmonary cirrhosis, have long been distinguished. All these, at least, are included in the generic name phthisis. When I say, "Have we a remedy for phthisis?" I mean, have we a remedy for this allied group of conditions,—conditions with which we have so often to deal as practitioners of the remedial art of medicine, which are due, it is true, to varying pathological changes, but which are marked in common by progressive wasting of the body, by progressing asthenia, by progressing diminution of respiratory capacity, and by fever of a hectic type. Every case of phthisis requires special study, and ought to be treated by no rule-of-thumb practice because it is phthisis. In one case anæmia is prominent and calls for iron or for arsenic; in another, continued but scanty hæmoptysis calls for ergot or for hamamelis; in another, a racking and frequent cough calls for opium or some of its derivatives; in another, dyspepsia calls for alkalies or for acids, for bitters, or for proteolytic or amylolytic digestives; in another, we have to aim at controlling profuse

perspiration or at checking an exhausting diarrhœa. Apart from these and other particulars, I suppose practitioners are agreed that cod liver oil, given either alone, or variously combined with other agents which tend to promote its assimilation or supplement it as a restorative stands at the head of remedies designed to advance the general nutrition of the phthisical. Have we any other general remedy? For a long time I trusted to syrup of the iodide of iron. This I gave up for a mixture of hypophosphites and iron—five grains of hypophosphite of lime, ten grains of hypophosphite of soda, and fifteen minims of syrup of the phosphate of iron, for a dose. This is a good combination, and I still use it. But chloride of calcium is my favourite general drug in pulmonary consumption. I have used it very extensively for many years, in hospital and in private practice, and I believe with great advantage. Perhaps you will say, in a commendable spirit of logical scrutiny, do you give chloride of calcium alone? I do not. I give it with cod liver oil, or with some emulsion of cod liver oil, in a separate watery solution, or I combine it, according to circumstances, with a vegetable bitter, or with morphia, or with ergot, and I believe I get better results with chloride of calcium in these combinations than I do with anything else in the same combinations. By the logical method of “concomitant variations,” in

the limited and qualified sense in which it is applicable to ordinary therapeutic experience, I have worked out to my own satisfaction the practical induction that chloride of calcium is, next to cod liver oil, our best general remedy in phthisis. Those of us who are in earnest, and who believe that therapeutic art—an art which includes, but which is much more than, the administration of combinations of the *materia medica*—can powerfully modify morbid processes, and can powerfully aid the *vis medicatrix naturæ*, and who think well of what we do, have done, have not done, and shall do for our patients, must be constantly arriving at conclusions, as the result of observation in our own practices, which modify, confirm, correct, or extend our therapeutic conduct. These conclusions may not always be framed by formal canons or able to bear the test of an exhaustive logical scrutiny, but, if such conclusions, which we are constantly forming for ourselves, are accepted by each of us, as reasonable and responsible men, as guides for our own therapeutic action, they are eminently worthy of communication to our brethren. On this conclusion I have long acted with confidence. My attention was first called to the value of chloride of calcium in phthisis by a paper in one of our medical journals, wherein it was stated that the drug was much used by the late Dr. Warburton

Begbie.* Scarcely mentioned, if noticed at all, in the therapeutic text books of our day, chloride of calcium has an old and well-established repute as a remedy for strumous glandular swellings. In 1808, Dr. James Sanders, of Edinburgh, in an important work on pulmonary phthisis, wrote :—
“ I think that I have ascertained that the muriate of lime has a more powerful effect in removing indolent serofulous tumours than any other substance used as a remedy.”† In subacute and chronic cases of pulmonary consumption I usually give ten grains of chloride of calcium, dissolved in three drachms of water and mixed with a drachm of glycerine, in a wineglassful of water or milk, twice daily, immediately after meals. I think the drug especially tends to check phthisical night-sweats, and that it favours increase of weight, and the drying up of pulmonary lesions. Of course I do not maintain it does these things in all cases. What I have stated are general conclusions, open,

* Dr. Warburton Begbie read an instructive paper on “ The Therapeutic Actions of Muriate of Lime ” before the Medico-Chirurgical Society of Edinburgh, on May 15th, 1872. This paper was afterwards published in the *Edinburgh Medical Journal*, and is contained in Sir Dyce Duckworth’s volume of “ Selections from the Works of the late J. Warburton Begbie, M.D., etc., ” issued by the New Sydenham Society, in 1882. Dr. Begbie wrote :—“ The cases in which I have had occasion most frequently to employ the muriate of lime have been instances of struma, the most notable feature of which was the enlargement of the lymphatic glands in the neck.”

† “ Treatise on Pulmonary Consumption.” By James Sanders, M.D., Edinburgh, 1808.

I am aware, to objection on the ground of their insufficient logical basis, but they are conclusions which have been and are for me amongst my most trusted indications for therapeutic action. In prescribing chloride of calcium, we must be careful to write the name of the remedy distinctly and in full, in order to avoid an error from which one of my patients suffered, namely, the substitution of "chloride of lime" for the drug I had intended to use.

V.

SOME POINTS IN THE TREATMENT OF THE SEVERER FORMS OF CONSTIPATION AND OF INTESTINAL OBSTRUCTION.*

Difference between constipation and intestinal obstruction.—Remedies in habitual constipation.—Signs of faecal retention.—Enemata.—Varieties of intestinal occlusion.—Symptoms of intestinal occlusion.—Treatment of intestinal occlusion.—Value of combinations of therapeutic resources.—When surgical interference is indicated.—Choice of surgical procedure.

I purpose to make some remarks upon several points in the treatment of the severer forms of constipation and of intestinal obstruction. I venture to do so because the subject of faecal retention and of occlusions of the bowel is a practical topic of the greatest importance to us as medical and surgical practitioners, because the

* A paper read before the Birmingham and Midland Counties Branch of the British Medical Association: published in *The British Medical Journal*, November 17th, 1883; since revised and rewritten.

details upon which I shall touch have especially and long engaged my attention as a physician, and because I hope I may be fortunate enough to lead the way to a discussion from which we may all reap substantial profit. It is not my intention to attempt anything like a complete examination of the whole question of constipation and intestinal obstruction. The subject, if treated systematically, could not be dealt with, even in a cursory manner, within the limits of the time at my disposal. You know that the literature of the subject is very extensive, that it reaches back to the earliest records of medicine, and that I could not give a summary of it within the compass of a readable paper; you know that intestinal lesions, and especially those pathological changes which tend to faecal obstruction and intestinal closure, have shared in being the objects of the analytical precision which is the leading note of the medicine of our century, and that I could not recount these details within a single sitting of our society. Keeping to what is practical in the pathology and practicable in the treatment of some of the commoner forms of constipation and intestinal obstruction, as I have met with them in my own reading and clinical experience, I purpose to ask you to consider with me the progress of our art in one of the most important and most striking departments of its usefulness.

We must avoid a common confusion of terms in the use of the familiar words constipation and intestinal obstruction. It is not strictly accurate to speak of intestinal obstruction, as some writers have done, as an exaggerated, an ultimate, form of constipation. It is quite true that some of the worst and most fatal forms of intestinal obstruction are usually long marked by a prodromal constipation, as, for example, cancerous constrictions of the larger intestine. But the phrases constipation and intestinal obstruction, when properly understood, do not merely mark different degrees of a similar result. They apply to different extents of the intestinal tube. Constipation concerns the large intestine only; intestinal obstruction, the whole of the intestines, small as well as large. Constipation is slow fæcal progress, in the large intestine, where alone true fæces are to be found. Intestinal obstruction is a grave disturbance of intestinal permeability in any part of the intestinal canal; it is practical impermeability of the intestines to the passage of their contents, either in the large or in the small intestine, in any part of the bowel, from pylorus to anus. "Constipation is essentially slow progress of the fæculent mass from the cæcum to the anus."* It is this, and nothing more than

* This sentence is quoted from a clinical lecture on "Retention of Fæces," by Dr. Matthews Duncan, published in *The Medical Times and Gazette*, Nov. 8th, 1879.

this, so far as the mere position of the difficulty concerns us, albeit the pathological causes of constipation, when organic, and when such as narrow the lumen of the bowel, are apt, in their extremest developments, to determine intestinal occlusion.

The manifold errors of habits, of effort, and of diet which tend to constipation are well recognised by our profession. In the discovery of some of these, and in their timely and persistent rectification, we can cure, without drugs, many of the slighter forms of *faecal* retention. We should make quite sure we exhaust these measures in the treatment of every case of habitual constipation. In the slighter cases, such non-medicinal treatment is usually sufficient for a good result; in severer cases, when drugs and instrumental aid cannot be avoided, all that well-ordered habits, well-directed efforts, and well-chosen diet can do should be regarded as the indispensable adjuvants of a more direct therapeutics.

Our pharmacopœias, officinal, non-officinal, and popular, are richer in purgatives than in remedies of any other class. I must not digress into a comparison of the relative values of our cathartic drugs, although the subject is a very tempting one. The practitioners of rational medicine have accumulated a vast store of precise and valuable information concerning the actions of purgative

medicines, and this important branch of therapeutics is still growing. Each of us has his favourite cathartics; if we have tried their adoption well, we should not lightly change them. For cases of habitual constipation which do not yield without drugs, my favourite remedy is Socotrine aloes. I have little faith in belladonna and none in nux vomica. Aloes is especially useful in the faecal sluggishness of sedentary persons. Properly given, the drug may be taken daily for years, without either losing its aperient efficiency or producing any but the best results. I give one, two, or three grains of Socotrine aloes in a pill, combined with a quarter of a grain of sulphate of iron and one grain of extract of hyoscyamus, at bedtime, every night.* I find out in each case the exact quantity of aloes required to produce one full alvine evacuation after the morning meal. In this combination the quantity of aloes will need readjustment from time to time, usually in the

* We are indebted to that veteran therapist, the late Sir Robert Christison, for the valuable suggestion of combining iron with aloes when we use aloes as a laxative. Neligan, in reference to the use of aloes in habitual constipation, wrote:—"Christison states that the cathartic property of aloes is much increased by its combination with sulphate of iron, and that its irritating action on the rectum is counteracted by combining it with the extract of hyoscyamus; both of which statements my experience fully confirms."—Neligan's Medicines, edited by Macnamara, 6th edition, p. 130.

direction of reduction.* I shall mention only one other drug for the class of cases I am now considering, namely, the American cascara sagrada. From my former experience of so-called new drugs, I have learned to employ such preparations with much caution and to recommend them with more. But I now venture to state that I have used cascara sagrada in many cases of severe habitual constipation, with marked success. I have given from fifteen to thirty minims of the officinal extract, thrice daily, adapting the dose by the result, and endeavouring to secure one, or at most two, dejections in each twenty-four hours.

The ordinary symptoms of extreme fæcal retention are well known. Our experience, in the main, justifies us in expecting that such symptoms

* I was led to adopt this combination of aloes and iron in the treatment of habitual constipation by reading a paper by the Rev. David Bell, M.D., which was published in *The British Medical Journal*, Nov. 5th, 1870, entitled "Remarks on the Beneficial Effects of Combining Tonics with Aperients in Chronic Constipation." Dr. Bell stated in this paper that he had tried various combinations of drugs in the treatment of constipation, and had come to the conclusion that the best formula was the following:—R. Aloes Socotrinæ, extracti liyoseyami, āā gr. xij.; quinæ disulphatis, gr. vj.; ferri sulphatis, gr. iv. To be well mixed and divided into twelve pills. Dr. Bell has found these pills to produce uniformly good results, without inconvenience. Dr. Kent Spender, of Bath, has kindly drawn my attention to his admirable paper on "The Therapeutics of Chronic Constipation," published in *The Medical Times and Gazette*, Feb. 19th, 1870. Dr. Spender recommends minute and frequent doses of watery extract of aloes, given in combination with sulphato of iron. He informs me he has treated cases of habitual constipation with pills of aloes and iron for the last twenty-five years, with excellent results.

shall be acute, or at least sub-acute, in their urgency and duration; and that they shall be associated with complete temporary absence of alvine dejections, or at least with very obvious insufficiency of such evacuations both in quantity and frequency. But we shall fall into error sometimes if we expect considerable faecal retention always to be marked in this way. Of exceptional forms of extreme faecal retention, I have met with two distinct varieties. In both the process of accumulation has been slow: but in one the graver signs of intestinal obstruction have at last become urgently and rapidly developed, as it were, as a climax; while in the other and rarer form of slowly developed faecal retention the condition has been chronic throughout, and the disorder has not perhaps been recognised until after a belly only distended by a dilated colon filled with faeces has been regarded as the seat of a huge tumour, the nature of which has been variously interpreted. I have known the extremest faecal retention, filling the belly, encroaching on the thorax, and displacing the liver, lungs, and heart, presenting itself as a chronic condition lasting for many years. Let me quote very briefly an extreme and very instructive instance of this kind from my notebook. In the year 1871, a medical friend sent a case to me at the Queen's Hospital, as one of obscure abdominal tumour, which had long

resisted treatment at two neighbouring medical charities, and about the nature of which he was in doubt, and desired my opinion. I found the patient a pale, ill-developed girl of fourteen. Her mother stated that when the child was only two years of age, it was noticed there was some enlargement of her belly. The child's bowels had habitually been confined, a week or more often elapsing without the passage of a motion. The evacuations had generally consisted of small portions of hardened fæces; but, from time to time, frequent and scanty liquid stools were passed. The quantity of urine appeared to have been normal; the appetite poor and capricious. The abdominal enlargement had gradually increased up to the time of the patient's application to me. I at once admitted the girl as an in-patient. I found she complained of occasional griping pains in the belly. She had never had any vomiting. Her motions were small in quantity and watery. The tongue was clean. There was no pyrexia. The body was fairly nourished. The abdomen was generally enlarged, and the lower part of the thorax expanded. The superficial veins of the abdomen were slightly enlarged. A solid tumour could be felt to occupy the whole of the right side of the abdomen. It had no distinct margin above, and reached, laterally, about two inches to the left of the middle line; below, the edge of

the hand could be readily passed between the tumour and the pelvis. The tumour was uniformly dull on percussion; palpation gave the impression of a doughy consistence, and firm and sustained pressure with the tip of a finger upon the mass produced a depression which lasted for some minutes after the finger was withdrawn. The heart was displaced upwards considerably; its apex was found to strike the chest-wall at a point one inch and a half above, and one inch to the inner side of the left nipple. The circumference of the abdomen at the umbilicus was thirty-one inches. The rectum was found to be largely distended, and filled with hardened fæces. The patient was ordered a pill, consisting of a grain and a half of Socotrine aloes, half a grain of extract of hyoscyamus, and a third of a grain of extract of nux vomica, to be taken with a drachm of sulphate of magnesia in one ounce of infusion of roses, thrice daily. An enema of cold water and table salt was given night and morning. Before the administration of the first enema, I freely broke up the contents of the rectum with my forefinger and the handle of a large table-spoon, and I removed a very large quantity of hardened fæces, together with three plum-stones. On the following day, two chamber-pots were literally filled to the brim with pultaceous fæces, and the abdomen was found markedly diminished

in size. On the next day, two chamber-potfuls of fæces followed the morning injection. On the following day, three copious motions were passed. On the next day, there were two free actions of the bowels, and it was noted that the abdomen was smaller and softer, and that the heart's impulse had fallen to the level of the left nipple. In three days more, the enemata were finally discontinued. Careful physical exploration failed to find any abnormal signs in the abdomen. Faradisation was ordered to be gently applied to the abdominal muscles daily. From this time the patient did well, without interruption, and was discharged fourteen days after her admission. She attended a short time as an out-patient, taking steel and an aloetic purgative, and remained well, without any fæcal reaccumulation. In this case you will notice that extreme fæcal retention, sufficient to displace the heart into the infraclavicular region, to distend the superficial veins, and to form a very large abdominal tumour, was unattended by vomiting, scanty urine, abdominal tenderness, or other local disturbance than "occasional griping pains in the belly." You will notice, too, the record of an important point in diagnosis. A large portion of the patient's abdominal cavity was obviously occupied by some solid mass. I had to decide upon the nature of the abnormality. I found that firm and sustained

pressure with the fingers, over the tumour, produced a depression in its mass which lasted for some minutes after pressure was withdrawn. This very exceptional physical sign is almost absolutely diagnostic of considerable faecal accumulation. The successful progress of the case illustrates, also, the value of using together a variety of therapeutic measures. In the treatment of faecal retention, the best results are only obtainable by the adoption of a well-considered combination of remedial resources. I did not rely on only one method of emptying the distended intestine. I broke up and dug out all the excrement I could reach through the anus; and I kept up the concurrent and continued use of aloetic purgatives, enemata, and faradisation.

I am afraid our profession does not adequately appreciate the immense advantages to be derived in the treatment of many of the severer forms of constipation and intestinal obstruction by the efficient use of the enema. In France, I understand, the enema is the routine domestic aperient. We do these things better in England. The custom of relieving slight constipation by an immediate resort to an enema has never become popular on this side of the Channel, and it is well it is so. My experience has led me to discountenance decidedly the systematic use of rectal injections in the ordinary domestic treatment of the slighter

forms of fæcal sluggishness. Such cases may be treated better, and especially with less tendency to chronicity, by other means. On the other hand, however, in the severer forms of fæcal retention, we ought always to use aperient enemata, and we must take care we use them efficiently. In persons past the meridian of life, and especially in persons of sedentary habits, what may be called simple fæcal retention is a very frequent form of constipation. In such persons this form of constipation is relatively very frequent, both as compared with other varieties of constipation, and also as compared with the same form of constipation at other times of life, and in individuals of other habits. In such persons coprostasis (a good old name for fæcal stagnation) is especially apt to produce complete intestinal obstruction. It is in these cases, especially, that life may be saved by enemata. I do not know any form of intestinal obstruction in which enemata can do harm. In most cases they take a chief place amongst our most potent means of doing good. In many cases which at first are unpromising, and even when the predisposing cause of the obstruction is some organic and incurable disease, we may repeatedly relieve a threatening fæcal accumulation, and long keep off a fatal fæcal stagnation, by the due use of enemata. It is, perhaps, not too much to say that enemata far surpass any other remedies in curative value in

the simple coprostasis of advanced life. Within the limits of this paper I cannot particularise all the practical details of apparatus, of quantity, quality, and frequency of intestinal injection, and the various manipulative niceties of administration, concerned in the question of the therapeutic use of aperient enemata. But I would take this opportunity of affirming that in all severe cases of constipation, and in all cases of intestinal obstruction, in which we use enemata, we can only administer our injections efficiently by means of the long tube of O'Beirne. Let me recommend O'Beirne's classical treatise on defæcation to those who are unacquainted with it.* Than from a study of its pages I do not think I have ever reaped more practical profit from any of my medical reading. The gist of O'Beirne's book is the recommendation of the long enema tube, which for fifty years has been known by his name. Never entrust the use of O'Beirne's tube to a nurse. The efficient passage of the instrument into and through the sigmoid flexure of the colon is a delicate and difficult operation, which the medical attendant ought always himself to perform for his patient. Much unnecessary detail has been taught about the composition of enemata. When we use an enema for the purpose of clearing the bowel of fæces and flatus the

* New views on the process of Defæcation, &c. By James O'Beirne, M.D., Dublin. 1833.

quantity of the injection is its chief quality. I am accustomed to tell my pupils that when they give an enema they should always ask themselves whether it is to be retained or returned ; if it be designed that the injection shall be retained, as in the case of a nutrient or sedative enema, its quantity can scarcely be too small. If, on the other hand, it is intended to move the bowels to the expulsion of their contents, the quantity of an enema can scarcely be too large. The quantity of an aperient injection is precisely so much of it as can be passed into the bowel. For such an enema to be so large as possible is only to be large enough.

I now ask you to favour me with your further indulgence while I consider very briefly the celebrated question of the treatment, and especially the operative treatment, of intestinal occlusion. I feel I need not apologise because, as a physician, I ask you to consider with me the operative treatment of intestinal impermeability. In the treatment of this most grave and anxious condition, it usually happens that it is not only the office of the physician to determine whether and when the aid of the operating surgeon is to be sought, but also to join with him in deciding upon the special operative measures to be adopted in each particular case. This question is no new one ; it is celebrated in the records of controversies which reach back to the earliest portions of our literature. Whatever

our conclusions on this question, they can only be accepted as provisional. From time to time its issues must be examined anew, so that they may be brought to the level of our latest curative advances. It is especially necessary that we should often recur to this important question now, when we are witnessing such rapid advances in the safety, precision, and scope of abdominal surgery. In these advances, as fresh and signal triumphs for our art, I humbly claim to share a common pride with my surgical brethren. I take this opportunity of offering my sincerest congratulations to this Branch upon the great advances which have been established in peritoneal surgery in our times, for with those advances the names of some of our members are honourably and indissolubly connected.

I shall not pause to enumerate the long list of lesions which may determine the clinical urgencies of intestinal impermeability, and which, by causing that most grave condition, may demand from us relief if life is to be preserved. As intestinal compressions, constrictions, degenerations, displacements, distortions, impactions, obturations, and stenoses, these manifold pathological conditions have been fully described. If, with practical purpose, we translate the anatomical causes of intestinal occlusion in their clinical manifestations and history, we shall find that they fall into three

fairly defined groups. *A: Causes which come into operation quite suddenly, and which lead at once to complete intestinal occlusion.* Here we have sudden compressions, displacements, and distortions, as all kinds of strangulations and torsions or kinks, some cases of intussusception, especially in children, and some cases of plugging by gall-stones. *B: Causes which manifest themselves acutely, but which do not give rise to immediate and complete occlusion, although they produce very grave disturbances of intestinal permeability.* Here we have partial strangulations of all kinds, many cases of intussusception, many cases of peritonitis, and cases of partial obturation by gall-stones and foreign bodies. *C: Causes which are developed slowly, and which give rise often for weeks, months, or years, to marked signs of impaired intestinal permeability, and which either lead to a series of subacute seizures of intestinal occlusion, yielding for a time to treatment, but successively increasing in severity and danger, or culminate in a single sudden and final attack of complete and unyielding obstruction, or lead to death in some indirect way, as by perforation, peritonitis, or asthenia.* Here we have intestinal cancers and neoplasms generally, strictures and stenoses of all kinds, chronic local or general peritonitis, compressions from the pressure of slowly growing tumours, and faecal impactions and chronic faecal retention from degenerative changes in the muscular coats of

the larger intestine.* These various conditions teem with practical interest, both in the niceties of their differential diagnosis, and in the details of their varying therapeutic requirements. Into these points I cannot now enter, but I would state generally that by a consideration of the age of the patient, of the history of his illness, of his special symptoms and physical signs, and of the results of our treatment, checked by a recollection of the pathological possibilities of intestinal occlusion and some accurate knowledge of their relative frequencies, we can usually make a practically correct diagnosis, both of the particular portion of the intestine which is affected and of the pathological nature of its lesion. I cannot, however, leave this part of my subject without reference to certain well-ascertained statistics and approximate generalisations which are of great practical importance, and which have often stood me in good stead at the bedside in the diagnosis of the kind and place of an intestinal occlusion. Firstly, it is generally true that sudden and very marked obstructions, such as strangulations, torsions, intussusceptions, and pluggings, affect the smaller intestine, while

* This classification is a modification and amplification of one to be found in Dr. Leichtenstern's valuable essay on "Constrictions, Occlusions, and Displacements of the Intestines," contained in Dr. Von Ziemssen's *Cyclopædia of the Practice of Medicine*. See English translation, Vol. VII., page 487, *et seq.*

more chronic but less accentuated difficulties of permeability, such as strictures, cancers, and intestinal degenerations, affect the larger intestine. Again, an intestinal stricture is a circumscribed diminution of the lumen of the bowel. It arises either from contraction of the mucous and sub-mucous tissues, or from the encroachment upon the intestinal canal of some new growth from the intestinal walls. The latter process is usually cancerous, the former is usually a consequence of ulceration. "Stricture may be met with in any part of the intestine, yet it occurs in different parts with very different degrees of frequency. The published statistics of fatal cases show that its occurrence as a fatal disease in the small intestine is comparatively rare (according to Dr. Brinton* in 8 out of every 100 cases); and that, as regards the large intestine, (to quote again Dr. Brinton's figures, with which those of other writers agree pretty closely,) out of 100 fatal cases, 4 are in the cæcum, 10 in the ascending colon, 11 in the transverse colon, 14 in the descending colon, 30 in the sigmoid flexure, and 30 in the rectum. Dr. Brinton calculates that stricture occurs three times in men to twice in women; and that the average age of death is $44\frac{2}{5}$ years."†

* Intestinal Obstruction. By W. Brinton, M.D., F.R.S. 1867.

† Obstruction of the Bowels. By J. S. Bristowe, M.D., F.R.C.P. Reynolds' System of Medicine, Vol. III., page 74, *et seq.* 1871.

From these figures we may gather the important practical generalisation that at least four-fifths of the strictures of the larger intestine are situated to the left of the middle line of the body. Again, excluding the grosser forms of hernia, of all the different forms of obstruction of the bowel, intussusception is the one which is "most commonly attended with the presence of manifest tumour;"* and further, excluding cancerous disease of the larger intestine, the discharge of blood *per anum* is characteristic of intussusception, and is generally present from the onset of the affection. Again, we have Mr. Jonathan Hutchinson's valuable generalisations, from which I select the following as being the most reliable, and therefore the most important. "When a child becomes suddenly the subject of symptoms of bowel obstruction, it is probably either intussusception or peritonitis. When an elderly person is the patient, the diagnosis will generally rest between impaction of intestinal contents and malignant disease. In middle age, the causes of obstruction may be various, but intussusception and malignant disease are now very unusual. If repeated attacks of dangerous obstruction have occurred, with long intervals of perfect health, it may be suspected that the patient is the subject of a chronic diverticulum, or has bands of adhesion,

* Dr. Bristowe. Op. cit., page 100.

or that some part of the intestine is pouched and liable to twist. If, in the early part of a case, the abdomen become distended and hard, it is almost certain that there is peritonitis. If the intestines continue to roll about visibly, it is almost certain that there is no peritonitis. This symptom occurs chiefly in emaciated subjects, with obstruction in the colon of long duration. The tendency to vomit will usually be relative to three conditions, and proportionate to them. These are, (1) the nearness of the impediment to the stomach; (2) the tightness of the constriction; and (3) the persistence or otherwise with which food and medicine have been given by the mouth.”*

No clinical spectacle is more terrible than that afforded by a case of acute and complete intestinal obstruction. All of us, probably, have seen some examples of it. In the midst of perfect health, without obvious cause or warning, or after some unusual and sudden muscular effort, or after a blow on the belly, or after a trifling diarrhœa or some slight constipation, or following some ordinarily insignificant error of diet, a vigorous adult is seized with severe pains in the abdomen. The pains are mostly griping and colicky in character, they usually come and go at short intervals, and they are usually referred to the

* Notes on Intestinal Obstruction. By Jonathan Hutchinson, F.R.C.S. *British Medical Journal*, August 31, 1878.

neighbourhood of the navel. Sometimes the pains are excruciatingly violent, or they are persistent, or they spread over the whole of the belly, or they are of a "bearing-down" character, and are attended by painful but fruitless efforts at stool. Acting on the familiar hypothesis that something has "disagreed" and requires clearing off, the patient usually forthwith takes a domestic purge. The pains continue and grow more frequent and severe, and the bowels remain unrelieved. At this stage vomiting generally appears, and a doctor is summoned. The gravity of the patient's condition is usually recognised, and pains are quelled and peritonitis staved off by opium, while efforts are made to open the bowels by enemata; sometimes, unhappily, the pathological possibilities are not adequately appreciated, and the stronger cathartics are injudiciously administered. Save for the passage of a little delusive flatus, or of the contents of the bowel below the difficulty, the belly remains ominously closed. Vomiting continues, and, in a variable time, the vomited matter becomes faecal in appearance and odour, while at first it consisted only of ordinary stomach-contents, or of a bilious watery fluid. The case grows more desperate; marked collapse soon declares the patient's increasing danger. The extremities chill, the respirations become shallow and frequent, and the voice fails and thickens, while the pulse is

small and rapid, the abdomen distended and drummy, and the face pinched, with pointed nose, sunken eyes, and thin, retracted lips. Hour by hour, and day by day, the sufferer grows worse, until, bathed in cold sweats, with parching thirst, frequent fæcal eructations, hiccup, shortening and shallower breathing, voice all but extinguished, dry brown tongue, Hippocratic face, failing and uncountable pulse, and mind unclouded to the end or gently wandering in the last few hours, death closes one of the saddest and sharpest scenes which human misery can show.

But the terrible and lethal condition* I have endeavoured to describe is not wholly hopeless. It is true it is very generally fatal, within six days at the most, yet patients have got well without surgical operation, even when internal strangulation has brought them to the very verge of death. Surgical art, I freely and thankfully acknowledge, has rescued not a few whom the skill of the physician has proved powerless to save, and this art promises, I believe, to include in a not distant progress a material reduction in the present high mortality of intestinal closure. “There is no cause of acute occlusion of the intestine,” writes Leichtenstern, “which cannot spontaneously disappear as well as originate. An intestinal knot can loose itself, an incarcerated or strangulated loop can become

* “*Morbus terribilis, creberrime letalis.*”—De Haën.

free, an invagination can become disengaged, compression cease, twisting or dislocation of the intestine with angular bend can straighten itself, a lodged gall or intestinal stone or foreign body may be dislodged and evacuated, and severe faecal obturation may be overcome.* But we must never forget, I would strongly insist, that the relative proportion of cases of spontaneous recovery from acute intestinal occlusion is very small indeed—so small as to support only a very uncertain hope of life in any particular case. I fear such a hope is often a harmful one, for I am afraid that its sympathetic exaggeration has sometimes inspired a disastrous inactivity, which has frittered away in fruitless endeavours and vain expectations the hour for a fair cast for life by surgical interference.

For want of time, I am compelled to pass over many important points in the treatment of intestinal occlusions, such as, for example, how far purgatives are to be given, if at all, the use of opium, of ice, of the best methods of feeding, of copious enemata, given while the patient is inverted or otherwise, of abdominal kneadings and manipulations, of warm baths, of rectal insufflation of air, of intestinal puncture with an aspirating canula. We do well to combine these measures variously, according to the special indications of each particular case, for clinical

* Leichtenstern. *Op. cit.*, page 508.

experience has abundantly shown that cases of obstruction of the bowel, even when they appear most unpromising, not seldom exhibit the best results of a wise combination of therapeutic resources. But I must now pass by these important topics, because I want to raise again the question, whether, when, and how the abdomen is to be opened for the relief of unyielding intestinal closure. If it be clear we have to deal with a case of acute intestinal occlusion, if a reasonable delay have been afforded for the operation of suitable therapeutic procedures and for a chance of spontaneous relief, if a due examination of all the hernial openings, including the obturator foramina, the sciatic notches, and the vagina, in the female, has excluded the evidence of external strangulation, if the rectum be found free, if full enemata, the passage of O'Beirne's tube, and exploration of the abdomen and loins by percussion and palpation have demonstrated that the difficulty is not in the colon, then the question must be decided whether laparotomy or laparo-ileotomy should be undertaken or not.

Laparotomy is abdominal section. Laparotomy may be performed in a case of intestinal obstruction with the view of finding and effectually relieving intestinal occlusion. Laparo-ileotomy is abdominal section, plus the formation of an artificial anus in the ileum. Against the

performance of laparotomy the following considerations may be urged, with various degrees of cogency, namely:—There are some cases of acute internal strangulation which cannot be relieved by laparotomy; consent is not usually given to the operation until the patient is so far *in extremis* that recovery is impossible; the intestine may be found so brittle that it is impossible to avoid fecal extravasation into the peritoneal cavity; the surgeon may not find any obstruction to relieve; the diagnosis may be incorrect; there is always a chance of spontaneous recovery; the operation in itself adds very seriously to the risk of death; the results of laparotomy are not such as to justify the performance of the operation. But, to these considerations it may be answered, the cases of acute intestinal strangulation which cannot be relieved by laparotomy, such as very complicated and extensive volvulus, are so rare, as to be practically unworthy of consideration in determining our action in any particular case; again, that laparotomy has hitherto been performed only when the patient has been in a hopeless condition is not an argument against the performance of the operation, but only a reason why the procedure should be adopted earlier; and again, the difficulty of diagnosis in cases of acute intestinal closure is daily diminishing, and is already sufficiently certain for all practical purposes, and

if the diagnosis of the nature of the exact lesion in any particular case be obscure, there is usually no difficulty in the prognosis that the patient will die unless relieved, and laparotomy gives him, at least, a chance of life. I have already said that the chance of spontaneous recovery is so small as not to be a just ground for putting off an operation when other means have had a fair trial, and have failed. With regard to the pouring out of fæces into the abdominal cavity, from the breaking of the intestine from extreme brittleness during a surgeon's search for an occlusion, I may say that I have seen such a case myself, and that I find instances of this fatal condition are recorded as having occurred in the experience of Billroth, Fergusson, and other eminent surgeons. In my case the patient was a young lady, in perfect and vigorous health up to her seizure with symptoms of acute intestinal closure; we did not put off the operation too long, and the case seemed quite favorable for the performance of laparotomy with a fair chance of success. As to the operation of laparotomy being a serious risk in itself, I suppose I shall be told that in these days it is not. But when we have duly weighed all these considerations for and against laparotomy, it seems to me that the strongest point in favour of the operation lies in the practical hopelessness of acute intestinal

occlusion if left to itself; while the strongest point against the operation is to be found in the records of its results. No very large statistics of the results of laparotomy have been collected; and those which have been published probably show a better result than the truth, because of the unfortunate tendency amongst us to think our unsuccessful cases unworthy of publication. I find, however, that Leichtenstern has collected seventy-nine cases, giving fifty-five deaths, or a mortality of seventy per cent. So far as cases within my own knowledge have enabled me to judge, I have been inclined to conclude that it is usual, in laparotomy, for a surgeon to find "something," and to relieve it, and that it has nevertheless been very usual for the patient to die shortly afterwards.

I have good authority for stating that laparotomies are an operation "far less dangerous than laparotomy, and more easy of execution."* Upon this point, which I regard of great practical importance, I hope I may be so fortunate as to elicit the opinion of my surgical brethren. I have a strong impression, based on some experience of the results of abdominal sections performed in cases of acute intestinal closure, that it is usually safest for the patient for the surgeon to follow the great example of Nelaton, and not to attempt a

* Leichtenstern. Op. cit., page 663.

search for the occlusion, with a view to its direct relief, but to be satisfied with the establishment of an artificial anus in the ileum, above the seat of intestinal stoppage. As compared with laparotomy, laparo-ileotomy is a much safer operation, and a much easier one. Even the relative disadvantage of a permanent artificial anus is not always entailed; for many cases of successful laparo-ileotomy have been recorded in which the artificial anus healed upon the spontaneous establishment of intestinal evacuations *per vias naturales*. I once met with a case of this kind in an elderly gentleman whom I saw in consultation with my friend and colleague, Mr. Oliver Pemberton. Excluding cases of insuperable intestinal obstruction in the colon and rectum, for which well established and special surgical procedures are required, there is, I think, only one class of cases of closure of the intestine for which laparo-ileotomy is unsuited, namely, for cases in which the occlusion is situated very high up in the bowel. These cases, which are very exceptional, are indicated, amongst other signs, by the absence of marked or of general tympanites.

My experience and my reading have led me to the following conclusions:—

If I had to deal with a case of acute intestinal closure affecting the smaller bowel, other than a case of external hernia, I should allow a fair

opportunity for relief by such remedies as opium, enemata, time, and belladonna; but I should by no means wait until the patient was *in extremis* before I urged surgical interference. If I obtained the patient's consent to such interference, I should hope my surgical colleague would perform laparotomy and relieve the occlusion, but only if its seat were so near at hand and its nature so clear that the operation could be thoroughly accomplished without much search amongst intestinal coils; but I should hope that he would rather perform laparo-ileotomy than subject the patient to the direct and collateral risks of a prolonged effort at disentangling his abdominal viscera.

But, as I have already said, our decisions on these topics can only be provisional, as, indeed, all our therapeutic decisions must be, if we be worthy the name of scientific practitioners. The scientific spirit accepts no result as final, but, with stern impartiality, presses steadily onwards to fuller truth, by ever assimilating the fruits of an ever widening knowledge. In such a spirit, if we are faithful to it, we shall still expect and still secure further vantage in our conflict with disease, and in this spirit our practice shall ever promise progress, and our art attempt and accomplish achievement.

VI.

ACCENTUATION OF THE PULMONARY SECOND SOUND OF THE HEART.*

*What accentuation indicates. — Clinical import. —
Prognostic value. — Therapeutic indications.*

Accentuation of the pulmonary second sound, or, to speak more precisely, accentuation of that portion of the second sound of the heart which is produced at the orifice of the pulmonary artery, and is especially heard in the “pulmonary,” as distinguished from the “aortic” area, although discovered and taught by the great Skoda in the earlier days of cardiac auscultation, is not generally recognised, if I may judge from the scanty references to it in text-books, and from my observations of its frequent neglect in the practice of stethoscopists, as one of the most striking and one of the most significant of the physical signs of disturbance in the mechanism and dynamics of the heart. It is a sign which is to be found in association, in causal relations which are tolerably

* A Clinical Lecture: published in *The British Medical Journal*, March 31st, 1883; since revised and re-written.

clear and approximately constant, with the commonest of the organic defects of the cardiac orifices and valves, and with the commonest consequences and complications of embarrassed cardiac action. Rightly interpreted, it is a sign which traverses the whole domain of practice, for it conveys reliable indications in the three chief divisions of our relations with a patient, inasmuch as it is significant alike in diagnosis, in prognosis, and in therapeutics. Skoda, with his usual tendency to over-refinement—that frequent fault of physicians—did not grasp the simplicity and singleness of the significance of accentuation of the pulmonary second sound. He observed the physical fact, but he went too far, and in one line in a wrong direction, in his speculation upon its import. He was wrong in his teaching, for example, as Dr. Walshe has pointed out,* that the presence of reinforcement of the second sound in the pulmonary artery will distinguish a systolic murmur at the left apex, caused by mitral regurgitation, from a murmur of like time and site, caused by friction of the blood against roughnesses on the inner surface of the ventricle. The essence of the matter is this: Accentuation of the cardiac second sound, as heard over the origin of the pulmonary artery, is an unfailing indication of

* Diseases of the Heart and Great Vessels. By W. H. Walshe, M.D., &c. Fourth edition, 1873, p. 93.

increased tension in the blood current in that vessel. In that it is this, it is a trustworthy sign, which a little consideration will enable you to understand, of a grave pathological condition; it is an unmistakable physical accompaniment of a portentous change in an area of the blood-circulation which is vital, and which is removed beyond the reach of those tactile and metric methods of exploration which are applicable in variations of tension in the systemic arteries.

In health, the aortic portion of the second sound of the heart predominates over that produced at the valves of the pulmonary artery. That is, the second sound is louder in the "aortic" than it is in the "pulmonary" area. By this statement I mean that the second sound is louder close to the right edge of the sternum, over the lower portion of the second right costal interspace, than it is close to the left edge of the sternum over the upper portion of the second left interspace. The blood tension may be raised in pathological states; either in the systemic circulation, of which the aorta is at the commencement, or in the lesser circulation, which passes from the right to the left sides of the heart, through the lungs, and at the commencement of which is the pulmonary artery.

Whatever raises the blood-tension in the aorta intensifies the aortic second sound; whatever

raises the blood-tension in the pulmonary artery intensifies the pulmonary second sound. What, then, is the clinical import of the variety of abnormal loudness of the second sound of the heart, to which I am directing your attention? Answering the question broadly, I say it is beyond dispute that an increased intensity of the pulmonary second sound is due to an increase in the blood-tension in the pulmonary artery, and that this heightened tension is due to some obstruction in the pulmonary or lesser circulation. The sign is clinically associated with organic and permanent lesions of the mitral valves and of the mitral orifice. Either insufficiency of the mitral valves, or narrowing of the mitral orifice, adds a distinct and new physical obstacle to the flow of blood through the lesser circulation. In so far as such an obstacle elicits increased force in the contraction of the right ventricle, by so much does it raise blood tension in the pulmonary artery, and consequently accentuate the pulmonary second sound. But, while this statement is strictly true as a generalisation, you must remember certain qualifying circumstances which may hold good in particular instances. Advanced mitral regurgitation, or advanced mitral stenosis, or both, may be present, and the pulmonary second sound may not be accentuated, but may even be less loud and clear than in health. This may arise from one of

two causes, or from a frequent combination of them, namely, from failure in the power of the right ventricle, or from the appearance of tricuspid regurgitation. In the course of mitral disease, when the force of the right ventricle at last fails to compensate for the obstacle on the left side of the heart, the blood-tension in the pulmonary artery inevitably fails and falls, and with it the loudness of the pulmonary sound inevitably declines and disappears. When also, in the course of mitral disease, the tricuspid valves, as so often happens near the end, give way, the pulmonary tension is at once lowered, and its physical sign disappears. Let me emphasise these important points by quoting some words of Rosenstein. He writes—"When the tension decreases in the pulmonary artery, the intensity of the second sound ceases; this takes place either when the right ventricle's force has been impaired by disease in the performance of its increased work, or when the right side of the heart is so filled by the increased stagnation that the ring of insertion of the tricuspid valve is widened, and the valve is no longer able to close the orifice."* I must also point out to you that, in comparing the pulmonary second sound with the aortic sound in cases of mitral disease, you must remember that the aortic

* Rosenstein. Ziemssen's "Cyclopædia of Medicine." English translation, Vol. VI., page 129.

second sound is likely to be relatively weakened by reason of the reduced systemic tension which mitral defects entail. As Dr. Walshe points out, there is a "pseudo-accentuation of the pulmonary second sound, from real weakening of the aortic second sound, through the lessened current and diminished calibre of that vessel, that follows on long-continued mitral regurgitation."* You must not fall into the error of mistaking a pulmonary second sound of normal loudness for an accentuated sound, because it co-exists with a feeble aortic sound. On this point, which undoubtedly is sometimes a difficult one in practice, you must look to an extended experience of cardiac auscultation to aid you. The recognition of variations in the tone and loudness of the heart's sounds is a refinement of stethoscopy, which only long practice can develop. It is only when, by patient clinical work, you have acquired in your minds a sure standard of the characters of cardiac sounds that you can readily detect deviations from their normal intensity.

So far as I have been able to judge from my own observations at the bedside, the presence or the absence of accentuation of the pulmonary second sound, or the presence of a high or of a low degree of such accentuation, is valueless as a differential sign in itself in the diagnosis of mitral

* Op. cit., page 100.

stenosis from mitral insufficiency. I know this statement is opposed to the teaching of some physicians and of some writers of acknowledged authority in cardiac diagnosis. Both varieties of mitral disease, whether they exist singly and pure or howsoever they may be combined, impose a morbid obstacle to the passage of blood from the right to the left side of the heart, and tend, *pro tanto*, to increase the blood-tension in the pulmonary artery. So long as this obstacle is met by a compensating increase of force in the contraction of the right ventricle, so long is the pulmonary second sound of more intensity than in health. The presence of such accentuation is not a sign which distinguishes one form of mitral disease from another, but it is a sign common to mitral lesions in general, which rises and falls in direct proportion to the vigour of the right ventricular systole. When in the backward march of the results of a mitral lesion, the saving force of the right ventricle becomes impaired by dilatation of its cavity or by degeneration of its walls, the pulmonary second sound loses its accentuation, and may become almost or quite inaudible.

You will now be able to appreciate the help which may be gained in the diagnosis, prognosis, and treatment of a given case of organic disease of the mitral orifice or valves, from observation of the condition of the second sound in the

pulmonary artery. Stating the case broadly, it may be said mitral valvular defects are generally the practically immediate and permanent results of acute endocarditis. Once established, the affection of the valves or orifice becomes a permanent defect, which never grows less, but which rather tends, by the organisation and contraction of inflammatory exudations, and by other well-known consecutive changes, to become more and more pronounced as time goes on. From the date of the endocarditis which first damaged the heart, there occurs a variable period of practically good health, or of quasi-health, but slightly impaired by certain of the less pressing signs of cardiac embarrassment. This period may vary in length from a few weeks or months to a few or many years, being determined by a variety of variously combined circumstances, such as the extent of original mitral damage and the degree of subsequent compensation, and the age, mode of life, social position, and habits of the patient. But, whether this period be short or long, there surely comes, sooner or later, an ultimate or penultimate stage, marked by failure of compensation and by dropsical complications, leading on to death.

Accepting this brief outline as a rapid sketch of the usual progress of mitral affections, let us answer this question :—What is the usual state of the pulmonary second sound in the progress of

such a case? When the mitral disease arises, that is, from the time acute endocarditis so affects the mitral orifice or valves as to set up a physical obstacle there to the normal progress of blood through the heart, the pulmonary second sound becomes accentuated, but only slightly so, for the most part, at this early stage. During the second period of quasi-health, that is to say, from the time of convalescence from the acute endocarditis until the onset of the later secondary complications consecutive to the mitral defect, the pulmonary second sound remains only slightly reinforced. You will generally find it as loud as the aortic sound, or a little louder, but not very markedly intensified. But towards the end of this second period, when the pulmonary tension is nearing the point when it shall overcome the compensating force of the right ventricle, the pulmonary second sound becomes very distinctly accentuated, and attains its maximum development. The sign is at this time of grave portent, for it is the sure index of an extremely heightened tension in the pulmonary circulation, which is not likely to be borne long; it is an unfailing sign that the pulmonary circulation is only maintained by an increased expenditure of force by the right ventricle, which cannot long be kept up. At this point a straw breaks the back of the labouring camel. A little added difficulty to the

circulation through the lungs, which usually comes as a bronchial catarrh, which would be trivial under some other circumstances, and the next, the ultimate or penultimate, stage of mitral troubles is ushered in. Compensation fails, and with it falls, *pari passu*, the accentuation of the pulmonary arterial sound. With failing compensation, viscera and surface become engorged with blood, anasarca gradually develops, and dropsical exudations begin to gather in the serous cavities.

This is the stage at which you often see patients admitted to my wards. With rest, good and carefully adjusted evacuants, and, above all, with digitalis, our great heart-restorer, many improve, lose the later complications of their mitral disease, revert to the second stage of quasi-health which I have been describing, and return to their occupations. As they improve, as rest, suitable food, evacuants, and digitalis, do good, you may notice the pulmonary second sound, which had waned before, wax strong again, surely marking the recovery of compensation in the propulsive power of the right ventricle, which is the essential factor in the patient's relief. Here observation of the pulmonary second sound is of inestimable service. With a rising sound, our treatment is doing good, and our patient is making progress towards recovery.

But the complications of this later stage of mitral disease, unhappily, cannot always be removed even once; and if removed once, or twice, or thrice, or oftener, there surely comes a time when all our remedies are at last of little or no avail. Be our treatment never so patient and skilful, the patient's condition remains stationary, or goes on from bad to worse. Here the compensating power of the right ventricle is finally and irretrievably exhausted; it is past all repair. Here the pulmonary second sound never rises under our treatment, but remains feeble to the end. Its continued feebleness, in the presence of dropsical complications, and in spite of our best therapeutic efforts, is a sure sign that the end is not far off, and that the patient is suffering his last illness.

VII.

REMARKS ON FLOATING KIDNEY.*

Cases of floating kidney established by post-mortem examinations.—Physical signs.—Case.—Causation of floating kidney.—Frequency in women.—Comparison of anatomical relations of kidneys.—Symptoms.—Treatment.

Floating kidney is a substantial reality, which must always be remembered in abdominal explorations. Its existence has been abundantly established by *post-mortem* examinations.

Some years ago a committee of the Pathological Society of London, consisting of Dr. Hare, Dr. Bristowe, Dr. Wilks, Dr. John Williams, and Dr. Wickham Legg, was appointed "to inquire into the matter of displaced, movable, and floating kidneys." From the report of this committee, which was published in 1876,† I quote the following paragraph: "Cases of undue mobility of the kidney verified by examination after death have

* A digest of two papers: Floating Kidney, *Birmingham Medical Review*, July, 1872; Remarks on Floating Kidney, *Ibid.*, October, 1883.

† Transactions of the Pathological Society, Vol. XXVII., 1875-6.

been several times recorded. One specimen was brought before our society sixteen years ago by Mr. Durham.* Dr. Priestley has described a case, under the care of Sir James Simpson, in which after death the peritoneum was found reflected over the posterior surface of the right kidney, thus allowing great motion on the right side.† Other instances have been recorded by Mr. Adams,‡ Dr. Iago,§ in which the state of the kidney was diagnosticated during life and verified by examination after death, Dr. Sawyer,|| Girard,¶ Urag,** and others.”

A floating kidney is a movable kidney, and something more. For a clear definition of this distinction we are indebted to Sir William Jenner. “A movable kidney is one thing; a floating kidney is another. . . . A floating kidney is a kidney that has a mesentery—a fold of peritoneum attaching it very loosely to the spine. A floating kidney, therefore, can be moved about to a considerable extent—to the extent of the length of its mesentery. A movable kidney can only be

* Durham, Transactions of the Pathological Society, 1860, Vol. XI., p. 142.

† Priestley, *Medical Times and Gazette*, March 14, 1857.

‡ Adams, *Ibid.*, p. 651.

§ Iago, *Ibid.*, 1872, Vol. II., pp. 328 and 409.

|| Sawyer, *Birmingham Medical Review*, 1872, p. 120.

¶ Girard, *Journal Hebdomadaire*, 1836, p. 445.

** Urag, quoted by Fritz. *Arch. Gen. de Med.*, 1859, p. 167.

passed up and down a little ; it slips a little under your fingers.”*

The largest statistics concerning movable and floating kidneys with which I am acquainted are to be found in the well-known treatise of Sir William Roberts on Renal Diseases. From these figures, from six cases of floating kidney which I published in the first volume of the *Birmingham Medical Review*, from several cases which I have met with since in my practice, and from other instances which I have found recorded, it appears that preternatural renal mobility may be either unilateral or bilateral, that it is more frequently unilateral than bilateral, that the right kidney has been affected about four times as often as the left, that floating kidney is much more common in women than in men, and that, amongst women, by far the larger number of the subjects of floating kidney have been women who have borne children.

When we palpate the abdomen of a person presenting a floating kidney, the patient lying in a recumbent position, with the abdominal walls relaxed, we can feel a swelling which is rounded, smooth, of the size and shape of a kidney, and which we can move in various directions, the

* Clinical lectures on the “Diagnosis of Extra-pelvic Tumours of the Abdomen.” By Sir William Jenner, Bart., M.D., &c., *British Medical Journal*, January, 1869.

movement being free and peculiarly slippery in its character. All the borders of the tumour can usually be easily defined by the fingers: the inner concave edge of the swelling, however, is often somewhat obscured. The displaced organ usually occupies a diagonal position, from above downwards, lying just below the free costal border, midway between the umbilicus and the last rib. The swelling can be readily moved in various directions; but it is most movable in a direction forwards, downwards, and towards the middle line; and next most movable in an opposite direction, namely, upwards, outwards, and backwards. The position of the tumour is affected by the position of the patient, the swelling descending when the upright posture is assumed, and falling towards the right or the left, according to the inclination of the body.

The respiratory movements, too, influence the position of a floating kidney. When the patient is lying down a deep inspiration may be necessary to bring the tumour forwards, so that we can feel it. Sometimes pressure on the renal region behind will suffice to bring a floating kidney forwards; sometimes pressure alone, and a deep inspiration alone, alike fail to do this, and both together are needed to bring the organ into prominence in front. The displaced kidney can generally be restored with the fingers to its normal position, but it

usually falls forward again when pressure is removed. I have met with two patients who had both kidneys floating freely. In one of these cases I could easily bring the two organs forward at the same time, and maintain their concave margins in contact in the middle line of the abdomen.

An opportunity of verifying or of correcting, post-mortem, a diagnosis of floating kidney occurs very rarely. The following is the only instance in which such an occasion has happened in my own experience :—

Early in the year 1870 I examined Mrs. Mary Ann H., aged 35 years; I saw her in consultation with my friend the late Dr. Hickinbotham, of Nechells. The patient was a spare woman, rather anæmic, and of nervous temperament. She had had seven children; her labours had been tedious, but natural. For six years she had suffered pain in passing her urine, with a constant desire to micturate. The urine was turbid, containing pus and phosphates; it sometimes contained a little blood. Sometimes the pain was very severe, and then she frequently passed some shreds of membrane in her urine, and occasionally this appeared in rolls as thick as a straw. After the passage of these substances she was usually better for several weeks. A sound introduced into the bladder indicated excessive

tenderness at one spot. A tumour, having all the characters of a floating kidney, was found in the abdomen, midway between the umbilicus and the anterior superior spine of the right ilium; this could be freely moved upwards, it could be easily grasped, and handling produced neither sickness nor pain. I regarded the pus as renal in its origin, and I suggested the existence of a calculus, and consequent pyelitis, in the floating kidney.* This woman died a few weeks afterwards, and Dr. Hickinbotham exhibited the right kidney at a meeting of the Pathological and Clinical Section of the Birmingham Branch of the British Medical Association, held November 25th, 1870. The following account of the case is taken from the *British Medical Journal*, December 24th, 1870:—

“Dr. Hickinbotham showed a specimen of abscess occurring in a movable kidney. The woman from whose body the specimen was taken had repeatedly suffered from great pain in the region of the bladder, with difficult and painful micturition; the urine being loaded with pus. She had never had any pain in the kidney itself until about fourteen days before death, when acute inflammatory symptoms came on, and, in spite of treatment, she sank and died on the 17th of September. The post-mortem examination showed general inflammation of the whole peritoneum;

* I was indebted to Dr. Hickinbotham for the history of the case.

and the right kidney, which lay midway between the umbilicus and the anterior superior spine of the ilium, was completely riddled by abscesses. The ureter was dilated and thickened. The bladder, except near the opening of the right ureter, was healthy."

The production of a floating condition of the kidney is an effect which is the result, doubtless, of the concurrence of several causes. Oppolzer thought that the affection is usually congenital, and this view seems to have been suggested by the lengthened condition of the renal vessels which has been usually found in these cases after death; this opinion, as far as my own reading extends, does not appear to have been fully shared by other writers on the subject. If the abnormality were congenital, it would be difficult to account for the disproportionate frequency of its occurrence in females. Cruveilhier thinks the practice of tight-lacing mainly contributes to the production of this affection.

In the paper by Sir William Roberts which I have before alluded to, child-bearing and tight-lacing are given as the most probable predisposing causes of the affection. "Becquet has proposed a somewhat novel theory for the production of movable kidneys in women. In the cases encountered by him, there was a striking coincidence of time between the displacement of the kidney and

the menstrual period; and he was led to believe that the kidney became congested and tumefied at these periods, and that displacement was the consequence of its increased volume and weight."* More or less rapid emaciation, occurring in persons who have previously been corpulent, has been considered, and with reason, to favour or of itself produce displacement of the kidney, by removing the fatty cushion which normally invests and supports the organ, and helps to retain it in position. Such a mode of causation probably existed in the case published by Mr. Adams.

There is abundant evidence to show that a blow or a violent concussion of the body may be the determining causes in some cases. Dr. Fleming attributed the mobility of the kidney in one of his cases to injury. Dr. Roberts quotes two cases, related by Henoch: in one, the right kidney became movable after a blow on the right loin; in another, the patient being a military officer, both kidneys became movable after a fall from a horse.

Many, perhaps by far the larger number, of the subjects of floating kidney are women who have borne children. All the examples which have fallen under my own notice have been observed at some period after child-bearing. To what extent a difficult and protracted labour may

* Urinary and Renal Diseases, Sir W. Roberts.

be concerned as a cause, I am unable to say. The powerful and prolonged contractions of the diaphragm which are incidental to such a condition would, doubtless, favour displacement of the kidney. I think, however, the circumstances which determine a liability to this affection arise rather as a result of the sudden removal of the pressure which the distended uterus exercises on the kidneys, in common with other organs within the abdomen. The tendency to falling forwards of the viscera, as a result of the impaired support afforded to them by the abdominal walls, in a woman who has borne children, appears likely to contribute to the production of floating kidney. Feeble women with lax and atonic tissues are probably more subject to this abnormality than those who are more robust.

The cause of the disproportionate frequency of a floating condition of the right kidney, as compared with the left, is to be found, doubtless, in the difference between the anatomical relations of the organs on the two sides of the body. Cruveilhier, as quoted by Dr. Roberts, remarks:—"If the left kidney is not so frequently displaced as the right, that is owing to the fact that the left hypochondrium, occupied by the spleen and the great end of the stomach, bears the pressure of the stays with much more impunity than the right." We must remember, also, that the kidneys are

moved a little by the respiratory movements. "The right kidney," writes Sir William Jenner, in the admirable lectures which I have already quoted, "is more depressed during deep inspiration than the left, probably from its relation to the liver."

The pressure of the liver—the pressure of its weight and the pressure of its diaphragmatic movement—contributes, unquestionably, to render the right kidney more liable to displacement than its fellow. The renal vessels, too, are usually a little longer on the right side—the artery especially—than those on the left; and we may observe that the ascending colon is not so closely applied to the right kidney, as the descending portion of the large intestine is to the left. The chief support of the spleen, the costo-colic ligament (the band of folded peritoneum which passes from the left angle of the transverse colon to the abdominal wall, opposite the last rib), helps in some small degree, perhaps, to increase the fixity of the left kidney.

These floating kidneys bear manipulation exceedingly well. Some authors allude to the production of a sickening sensation when the tumour is squeezed. I cannot say that I have observed this; but firm pressure, undoubtedly, causes pain.

The patients usually experience a feeling of dragging, uneasiness, and of weight in the abdomen,

which they refer to the tumour, and which may be increased by standing for some time, or by exercise, or which may never be felt except under such circumstances. Sometimes movements of the displaced organ are perceived by the patient, and then they may give rise to delusions, which we find great difficulty in dispelling. In one of the cases I have given, the patient persisted, in spite of all we could say to the contrary, in believing the movements to be those of a child. Dr. Roberts alludes to a similar case. In uncomplicated cases the secretion of urine is always healthy, neither is there any interference with micturition. The irritating condition of the urine was quite a sufficient cause for the frequent desire to empty the bladder, noticed in Dr. Hickinbotham's case.

The dragging and uneasy sensations may generally be relieved. Sometimes they are completely removed by wearing a tolerably tight, elastic, abdominal bandage. Anæmia, or dyspepsia, or disorders of the uterus, must not be overlooked, and suitable means must be adopted for their removal. Treatment of a tonic nature may be pursued with great advantage. When the abdominal walls are weak and relaxed, shower baths, rest, and chalybeates are indicated. All constriction of the lower part of the thorax, by stays or waistbands, or the like, must be avoided.

The action of the bowels must be carefully regulated; constipation, and the consequent straining, invariably aggravate the mischief. Violent exercise, such as riding on horseback, dancing, etc., should be prohibited. Rest and good food do much for our poorer patients.

Floating kidney, of itself, can never shorten life; it usually persists for an indefinite period. It is necessary that we should clearly understand this curious abnormality, and be able to form a correct diagnosis, that we may remove all alarm from the mind of the patient, and prevent the adoption of injurious measures of treatment.

VIII.

THERAPEUTIC PROGRESS.*

Comparative backwardness of Therapeutics.—Difficulty of therapeutic inference.—Therapeutic triumphs.—Neglect of therapeutic teaching.—Danger of false theories.—Right relations of science and practice.—Recent therapeutic progress.—Scope of therapeutics.—Conditions of therapeutic progress.—Physiological research in therapeutics.—We must yet be empirics.—Progress proceeds by the discovery of the unknown and the perfection of the known.

Gentlemen,—It is my first duty to ask you to accept my hearty thanks for the honour you have done me to-day, in my election to the presidency of our Branch of the British Medical Association. I feel you have called me to a high responsibility in placing me, by your favour, at the head of one of the largest and most influential divisions of the largest professional society in the world. You

* A Presidential Address, delivered at the Annual Meeting of the Birmingham and Midland Counties Branch of the British Medical Association, held in the Birmingham Medical Institute, June 25th, 1885: published in *The Medical Times and Gazette*, August 8th, 1885.

have called me to succeed many distinguished predecessors. Let me assure you I appreciate your confidence and consideration to the full. I undertake the duties you have placed in my hands with a sincere desire and with a single determination to do my best to justify your choice.

In choosing a subject upon which to address you, I remember that former presidents have most profitably engaged our attention upon a wide variety of topics. I can recall many brilliant addresses, some of which have not been without marked professional and public influence, and consequence of good, in the initiation and support of manifold improvements—of improvements in our relations, in our duties, in our powers, and in our practice. We have listened in succession to the able exposition of such important questions as the progress of ophthalmic surgery, the care and cure of the insane, the management of habitual drunkards, and our due relations to the sick poor and to the benevolent public through provident organizations and medical charities; we have heard, too, of the marvellous developments of surgery in our times, of the political duties of our profession, and of our moral and sanitary responsibilities. I am venturing to-day, gentlemen, to break other ground in asking you to return to a theme older, perhaps, than any of these; to one older, but to one which is ever

new, for it touches us all in our daily work as practitioners, namely, the *therapeutics of disease*; the use of remedies for the cure and relief of our patients, and especially the remedial actions of medicines. The subject is a great one, and its adequate consideration is far beyond the scope of an inaugural address. I can only attempt now to take up a part of it. About the interest of therapeutics to us I can have no doubt. Let us regard to-day, gentlemen, if you please, the particular question of our therapeutic progress. Are we making real progress in the treatment of disease? How may we improve and quicken our advancement? What are the obstacles to our progress, and how may we hope to overcome them?

I remember that a great medical authority, and a distinguished modern physician, the late Sir Thomas Watson, in his inaugural address at the foundation of the Clinical Society of London, in the year 1867, said:—"The greatest gap in the science of medicine is to be found in its final and supreme stage—the stage of therapeutics." And although, in the nineteen years which have passed since this declaration was made, the healing art has achieved many substantial and practical advances and developments, we must all of us still feel, I think, and often feel acutely as practitioners, in our daily application of remedies

for the cure and relief of disease, that we want a knowledge more exact, a scope more enlarged, and indications more direct and more successful, of the means by which morbid processes may be prevented and extinguished. How can the art of "treatment" be placed upon a broader and sounder basis,—upon a basis less shifting, less empiric, more demonstrable, more effectual, and more scientific? We thankfully rejoice in the advances of physiological and pathological science. These advances are good in themselves, and we welcome them with a hearty expectation that they may lead us to improvements in our practice. But we are disappointed that therapeutics lags behind. Why have these sciences of life and of death outstripped the science of healing?

There can be no doubt that the enormous difficulty of accurate therapeutic inference is the chief obstacle to the establishment of therapeutics as a scientific system, in the strictest acceptation of that term. This difficulty has never been overcome. It inheres intrinsically to the subject, and the subject is of unsurpassed complexity. In ages of scientific progress we have reduced this difficulty a little, and we shall yet surely reduce it still more; but shall we ever remove it? In a therapeutic inference we have to conclude about the action of a given drug upon a living human body, in a state of disease. The question is easy, but we

cannot complete the equation. We can cite the question thus clearly, but we cannot *state* the equation; for one reason, because we cannot state the great unknown quantity it includes. The terms are life, a disease, and a drug. In the whole range of human research there is no problem more difficult of exact solution than the question, which can be so simply stated—Does a certain drug cure a certain disease? To the uneducated the answer may seem an easy one; but the keenest logician of our time, John Stuart Mill, has put precisely this question as an illustration of the most intricate class of problems which the human intelligence can attempt to unravel, as the extremest instance he can imagine, when he “clothes in circumstances” the inherent and often insuperable difficulties which beset our reasoning when we have to deal with causes which are plural and distant, and with effects which are intermixed and many.

But it is our business as practitioners to “treat” patients and the disorders that are in them, to preserve and restore to our patients their activity, to assuage their sufferings, and, if it be possible, to cure them. As faithful practitioners, in our daily dealings with the practical and the concrete, the considerations I have just adduced must not weigh upon us unduly. I have been speaking only of the difficulty of satisfying the

severest canons of formal logic in a scientific inference about the cure of a disease by a drug. Outside the scope of such a demonstration, much of solid therapeutic achievement, much of priceless worth to our race, remains in the arts of medicine. Surely do we cure many diseases, and surely do we mitigate many more. Nor need we always cure their diseases when we save and restore our patients. Does the mariner cure the wind and the waves when he guides his ship in safety through a storm which would have overwhelmed her if he had been less vigilant and less skilful? And so we can have no doubt that in many diseases, the duration of which we cannot shorten, as in some of the specific fevers, we can so *manage* the patient as to make the issue for him recovery instead of death.

But happily, also, we can have no reasonable doubt that we really cure many diseases; to feel sure of this we need not wait to satisfy the sterner requirements of logical proof. Paralysing doubt melts into confident action without waiting for the later demonstrations of final certainty. Can we doubt that we cure syphilis with mercury, or ague and its allied neuralgias with quinine, or many forms of anæmia with iron, or acute rheumatism with salicylate of soda, or some skin diseases with arsenic? And in diseases which we do not yet claim strictly to cure, are we not

sure that their manifestations are largely within our control? Think of nitrite of amyl and of nitro-glycerine in angina pectoris, of iodide of potassium in asthmatic dyspnœa, of the bromides in epilepsy, of digitalis in affections of the heart, and of venesection, or of chloroform, in convulsions. And again, the secretions and evacuations of the human body, if not wholly within our control, are largely under the influence of our therapeutic means. And is not pain, the commonest and the most urgent of all the expressions of disease, almost absolutely within our power?

But there is another great difficulty in the way of progress. Now and here, at this time, and in this country, there is another serious obstacle to therapeutic progress. If we watch the current methods of medical education, we shall soon observe that the details of practical therapeutics are not, as a rule, sufficiently dealt with by our teachers. The examining bodies, in their curricula, unfortunately join *materia medica* and therapeutics to form the single subject of one short summer course, and present it to the student in his first year, when his acquaintance with disease and with patients has scarcely begun. The art of treatment is now a neglected branch of medical instruction; its neglect is not often felt by the pupil until he becomes a practitioner.

Now that medical students are no longer apprenticed, to learn in the practice of a surgery the art of applied therapeutics, but pass at once from the school desk to the hospital ward, they especially need long and careful training in the science of treatment and in the art of prescribing. The elaboration of the scientific details of the medical curriculum, as contrasted with the practical work amongst remedies required by the obsolete custom of apprenticeship, (a system which had many practical advantages), — the prevailing elaboration of the scientific details of the medical curriculum has too often crowded therapeutics out of the cognizance of the modern student of medicine. The duration of the course of lectures on *materia medica*, as now required by most of the licensing bodies, is so short, and the period when these lectures are attended, namely, in the student's first summer session, is so inopportune, that justice is not done to the important range of practical subjects, such as pharmacy, pharmacology, the physiological actions of medicines, and the art of prescribing, which are huddled and hurried into this part of the current curriculum. Hence the young practitioner, when he has taken his diplomas and left his school, without sound training in the discrimination, combination, and application of remedies, too often finds himself imperfectly prepared for

the practical responsibilities of his position, and he is in risk of abandonment "to the alternative of two great evils—a feeble and servile routine, on one hand, or a wild and lawless empiricism on the other." In these difficulties hard work, a clear head, and a good conscience may still save him; but he may be tempted to a treacherous refuge by the easy charms and attractive nostrums of proprietary pharmacy, or seduced by one or other of those notorious therapeutic generalisations which can still captivate the ignorant, though they be tottering to their fall between the crutches of knavery and credulity.

It would be easy to gather from the history of our art abundant instances of how much an accurate knowledge of remedies has been obscured and retarded by superstition and by credulity, by scepticism and by caprice, by fashions in diseases and by fashions in remedies, and by false doctrines founded upon false theories of morbid processes or upon false theories of the properties of medicines. In our times, with a sober, yet hopeful, temper in our judgments upon remedies, we may expect to escape many of the errors of the past and to help forward a sounder therapeutic progress. We must seriously and patiently examine again our old remedies, and search for new ones, by the best resources of modern scientific precision; and we must set ourselves

to do this with no superstition, and without too much credulity, and above all, without too much scepticism. It would be easy to point to a pedantic scepticism which, in our days, has sometimes found disastrous expression in our schools, as an egotistic inflation of therapeutic ignorance, or as the premature offspring of our exacter pathology. Credulity has been well defined as belief without reason; scepticism is reason without belief: and history has generally shown that a race of credulous believers begets a generation of unreasonable doubters. But our history warns us that some of the most striking errors of our art have sprung from hasty and false generalisations as to the properties of remedies or as to the nature of morbid processes. Now, as ever, we must be watchful lest we gaze through glasses coloured by the deceitful hues of false theory and premature conclusion. Possibly an unproved bacillary pathology may tempt us into premature bacillary therapeutics. As therapists we have noted that medical science has lately presented one of its periodic revisions of the pathology of pulmonary phthisis. It has introduced to us a living and material germ, and labelled it the tubercle bacillus, and it has found this organism as a newly recognized concomitant of tubercular processes, or, as some would claim for it, as the characteristic, ultimate, and peculiar structural

element of tubercle itself, or even as its infective, material, and potential essence. It is now too soon to attempt to sum up the true value of the well known and important investigations to which I refer. I may say, however, in passing, that I have a strong suspicion, which many of my brethren, doubtless, have also felt, that the tubercle bacillus will soon subside into obscurity, and that it will turn out, at the most, to be only an accident of tubercular processes. Of course I am using the word "accident" as opposed to "essence," strictly in its well known logical sense. Before long, possibly, our scientific brethren will have kindly found for us a special micro-organism, appropriately and specifically named, for every disease which presents an organic basis in which micro-organisms can flourish. Then will science once more react from error by proving too much, and the etiological bacillus will be lost in its universality.

In view of this immediate and particular question of the structural etiology and essential pathology of tubercular diseases, it is well for us, as therapists, if we would secure our progress, to realize our right attitude towards the sciences which underlie our art, and especially towards that great department of inductive knowledge which is distinguished as the science of medicine. The final test of inductive truth is the touchstone

of practice. In medical practice we must prove the generalisations of medical science. Clinical experience is the balance which alone can weigh their value. As faithful practitioners we must always welcome new scientific truths, and reflect their due influence in our practice. We must ever watch and support the labours of those who are breaking new ground in the elucidation of morbid processes. We must watch them with an expectation which hopefully waits for clinical result, and with an interest which tends to action. But our experience has taught us to be cautious in the acceptance, not of new scientific facts, but of new scientific generalisations. We remember that fifty years ago the essence of phthisis was the tubercular granule. Twenty years ago the ingenious Niemeyer revised and developed the teachings of Addison, and elaborately worked out the catarrhal and broncho-pneumonic hypothesis of pulmonary consumption. Then, thereafter, came the "giant cell" of tubercle. And now Koch asks us to contemplate the tubercle bacillus. Premature generalisation from particulars seems to be a very persistent tendency of the human mind, and it is a tendency which education and experience alike teach us to resist. Of all the errors which have retarded the progress of the science and art of medicine, premature generalisation is the chief. When we would ascend from

particulars to generals, credulity is our bane, and scepticism our duty. And in medical science we must bring our conclusions face to face with particular and living cases of disease, and see if they be true then. While clinical medicine, to be progressive, must ever revise and recast its practice by the truths of medical science, the generalisations of medical science must find in the particular readings of clinical medicine the truest tests of their validity. If our science sometimes restrains us when our art is going astray, it is our art only which has often shown us when our science was falling into error. In medicine, as in morals, practice is the test of principle.

We shall brighten our faith in the possibilities of therapeutic progress when we recall the marvellous practical advances which the healing arts have achieved within the last five-and-twenty or thirty years. I cannot now attempt to cite these advances in detail, but I believe a critical examination of them would show that our power to cure and to alleviate disease has made more substantial progress within the time I have named than it has gained within any similar period in the history of medicine. Our remedies have grown in simplicity and in range, in number and in precision. We have developed the uses of some good old drugs, and we have learned the uses of some good new ones. I am sure many instances of such

progress at once occur to your minds. Here are some prominent examples. The salicylates have become established beyond question as powerful remedies in pyrexial rheumatism. Thirty years ago the use of the alkaline bromides in epilepsy and its allied disorders, now so familiar to us, was quite unknown. In nitrite of amyl and in nitroglycerine have been found agents which can largely prevent, and which can markedly assuage the agonising paroxysms of angina pectoris. In chloral, in croton-chloral, in iodoform, we have found serviceable new drugs. In our time, too, the indications for the use of digitalis as a cardiac sedative, and as a cardiac tonic of marvellous power, if rightly employed, have been clearly worked out, and incorporated amongst the most reliable staples of our art. What may be called the local treatment of diseases of the respiratory organs by the use of inhalations has been largely developed in recent years. Think, too, of the therapeutic field which we owe to the hypodermic syringe !

And now, gentlemen, if you will kindly bear with me a little longer, let me invite you to look forward. Surely the prospect, the prospect of our remedial art, is encouraging. Here and there the haze obscures, and here and there it hangs thick and low, but the clouds are clearing, and we can see many a broadening gleam of bright blue sky.

Let us remember the immensity of our prospect. The potentiality of our art is only bounded by the physiological possibilities of human life. Our art aims at the prevention and the cure of all disease. Towards this consummation, so devoutly to be wished, it is sure to grow. The lines of its development are plain, and we know them well. Only by slow experience, and only by the labours of many hands, can our progress be maintained. Little by little shall our knowledge surely grow, but only by the experience of reliable observations, infinitely multiplied and laboriously compared. And from another aspect, and from one more immediately practical, we recognize the vastness of the scope of therapeutics. I need not remind my hearers that the art of therapeutics is not merely the administration of drugs. It is much more than this; it includes every agency and circumstance which can favourably influence disease. It includes dietetics—what a patient ought to eat, what he ought to drink, and when and how; it includes balneology—an ancient therapeutic system which has a greater future; it includes climatology; it includes the physical resources of mechanics—a mine of boundless wealth, which in means for the evacuation of morbid collections, and for securing rest, immobility, and support, has achieved such brilliant results; it includes electricity, and other forms of

gymnastics ; it includes the regulation of occupations, pursuits, and amusements ; and it includes many details of practical education, in their physical and psychical bearings upon growth and stability.

I wish the general mind of our profession were more clearly directed towards the pursuit of therapeutic progress. If the plainer conditions of such advancement were more generally recognized, and more generally kept in recollection, we might soon reap some substantial improvements. We must never forget that we cannot pursue therapeutics successfully to the exclusion of other branches of medical art and science. We cannot be therapeutic specialists. It is clear to us all that we must know more, and a great deal more, of the causation of disease before we can construct a therapeutic science. Hence, as therapists, we must watch and welcome all investigations into the nature and origin of pathological processes. We must cultivate, too, the art of clinical observation, and especially the art of diagnosis, for it is obvious that an accurate discrimination of the character of a disease must precede, in any particular instance, its intelligent treatment. And here, too, I think we shall agree that there is a distinct danger from undue specialism in practice. While what is known as specialism in practice has done much to advance

our remedial arts, and while specialism within certain limits is wisely accepted by our profession as a sound rule of conduct, I think we shall admit that there is an undue specialism against which we must guard, because it is a dangerous obstruction to real therapeutic progress.

But our therapeutic progress cannot rest upon clinical experience alone. Physiological research into the precise details of the powers of remedies, and such research directed towards the perfection and discovery of remedies, has already yielded good fruit in practice, and is full of promise. The two broadest and directest lines of therapeutic progress lie in these two fields of work, in clinical experience and in physiological research. Each supports the other, and neither can stand alone. While clinical experience suggests specific wants, which physiological research may endeavour to supply, physiological research supplies new agents which clinical experience may test in practice. Clinical experience reveals the therapeutic effects of medicines; physiological research discovers only their physiological actions. The therapeutic effect of a medicine is its remedial efficacy in disease. The physiological action of a medicine has no necessary connection with its therapeutic powers; it is an effect it produces upon a living and healthy body. We shall fall into error if we assume that there is always a necessary connection

between the physiological actions of a medicine and its therapeutic effects. There are some agents which have marked physiological actions, and yet are poor in therapeutic powers; and there are some remedies which have accepted therapeutic efficacies which exhibit scanty physiological manifestations. If we examine the matter closely we shall find that in many instances the therapeutic effects of a remedy and its physiological actions are, so to speak, two distinct, but not separate, sides of its character. But, however these things may be, we cannot doubt that the more we learn about the physiological powers of remedies the more likely we are to understand, the more likely we are intelligently to direct, their therapeutic employment in our practice. So, if we would make progress in our powers to cure disease and to relieve physical suffering, we must heartily help and patiently watch the physiological investigation of the actions of old and new remedies, and of old and new agents which may possibly become remedies. Our experience tells us that we shall often find that an agent which has particular and well marked physiological powers has also the capacity of a remedy with distinct therapeutic actions. In this way physiological research suggests for us and for our patients new remedies, or new applications of old ones, and hands them on to clinical experience

for test and for proof. Physiological research yields us a perennial spring of therapeutic progress—a spring in which our art may perpetually renew its vigour. Here is an abundant source from which we may draw an exacter knowledge. Here shall the art of medicine become less empiric. Here shall our science become more practical and our practice more scientific.

There is another condition of our work in the treatment of disease about which there has been much misconception, but about which we can scarcely miss agreement if we weigh the case carefully. It is this: Whether we like it or not, we must yet be mainly empirics in our practice. We must not be above being empirics. I do not mean empirics in any bad sense of the word. I do not even mean that there is any real opposition between true empiricism and scientific practice. The great bulk of our therapeutic knowledge is as yet empirical, and as empirics, though as rational and scientific ones, we must administer it. By this empiricism I mean, and mean only, a knowledge which is founded upon experience. I mean a knowledge which grows from and with experience, and which in this sense is empiric, however scientifically it be applied. In much of our therapeutic work it is experience which prompts our action, while experience alone can test our results. Much of what we do we cannot explain, in the

scientific meaning of the word explanation, so we lean upon experience, and trust with an empiric faith much that we know to be true though we cannot understand it. We expect much of further gain to our art from the discoveries and developments of physiological research into the actions of medicines, and such research has already found us some valuable remedies, which an *a priori* reasoning has applied in practice, and which experience has confirmed. But, in our time, experience must yet be our chief guide in therapeutics. Here is a specific question, which we have to answer every day. Why do I give this medicine to this patient? Not because it has such and such physiological effects, and I expect, therefore, that it will do good; but because I have before found its administration attended with advantage under similar circumstances; and this experience *satisfies* me, and gives me confidence in using it again, until I know of a better remedy.

Progress has two sides. It advances with a double front—by the discovery of the unknown, and by the perfection of the known. What the unknown has in store for us we cannot say, but surely a mine of therapeutic progress lies ready at our hands in the perfection of the remedies we already know. The discovery of the unknown is reserved as a rare and great reward, as the guerdon of the few. Towards the perfection of

the known we all can work, and none shall miss his prize. The discovery of the unknown bears fruit late, seldom, never, and splendidly; the perfection of the known bears fruit at once, always, continuously, and in bushels. Many good remedies are not fairly tried. A remedy has not failed when it has only been employed improperly. Let us study again, and more closely, and by modern scientific methods, many a good old drug, such as arsenic and antimony, mercury and hemlock, sulphur and turpentine. Let us watch, too, the marvellous developments of modern chemistry in their bearings upon our medicines. With a wide eye let us watch the sciences which are ancillary to ours, that we may translate their advances and resources into practical therapeutic utilities. Let us be reading, and reading old books, as well as doing other work. If we turn to the therapeutic literature of the past we shall find not only that the fittest remedies have not always survived, but that some of our most striking modern curative triumphs have been gained by old remedies which had long been forgotten, as, for instance, in the treatment of pyrexia by the affusion of cold water—a revival and not a survival—a revival of the therapeutics of Currie, of Liverpool, at the beginning of this century, and of others before him. Venesection is worthy at least of partial revival, and is sure to come into vogue again. We might

too, give point and precision to much that we know if we revived a little of the evacuant treatment of our predecessors—a treatment which, although based upon a superseded humoral pathology, was often sound in its practical results—and, casting aside much of the modern tonic rubbish which so easily besets us, studied the unloading of the viscera, and cultivated a robust therapeutics, based upon an accurate survey of the vital individuality of our patients.

INDEX.

	PAGE
Accentuation of the pulmonary second sound	70
Accentuation of the pulmonary second sound, clinical import of	74
Accentuation of the pulmonary second sound, Dr. Walshe on	74
Accentuation of the pulmonary second sound, prognosis in	79
Accentuation of the pulmonary second sound, thera- peutics in	82
Affusion of cold water in insomnia	19
Alcohol as a cause of insomnia	13
Alcohol as a hypnotic	18
Alcohol as a remedy in insomnia	13
Aloes in the treatment of habitual constipation... ..	47
Begbie, Dr. Warburton, on the use of chloride of calcium	41
Bell, Dr. David, on the treatment of habitual con- stipation	49
Brain, state of, in insomnia	4
Brain, state of, in sleep	3
Brinton, Dr., on intestinal obstruction	61
Bristowe, Dr., on intestinal strictures	62
Bromides, use of, in insomnia	18

	PAGE
Cascara sagrada, use of, in constipation	49
Causes of insomnia... ..	1
Causes of intestinal occlusion	58
Chloral, use of, in insomnia	16
Chloride of calcium, administration of	42
Chloride of calcium, Dr. Sanders on the use of ...	41
Chloride of calcium, therapeutics of, in phthisis ...	38
Chloride of calcium, Dr. Warburton Begbie on the use of	41
Christison, Sir Robert, on the treatment of habitual constipation	48
Coffee as a cause of insomnia	14
Colchicum, use of, in insomnia	21
Constipation, aloes, use of, in	47
Constipation, definition of	46
Constipation, enemata in	54
Constipation, habitual, treatment of	47
Constipation, O'Beirne's tube in	56
Constipation, treatment of... ..	44
Consumption, chloride of calcium in	38
Consumption, medicinal treatment of	38
Consumption, pulmonary, relations of, to phthisical laryngitis	25
Consumption, remedies in... ..	39
Coprostasis	55
Counter-irritation, use of, in phthisical laryngitis ...	32
Cure of insomnia	1
Deafness, in insomnia	9
Defæcation, O'Beirne on	56
Deglutition, painful, in phthisical laryngitis	28
Diagnosis of fæcal retention	49
Diagnosis of floating kidney	86
Diagnosis of intestinal occlusion	63

	PAGE
Digitalis, in insomnia	18
Duncan, Dr. Matthews, on retention of fæces	46
Dyspnœa, in phthisical laryngitis... ..	28
Empiricism	114
Empiricism, scope of, in practice	114
Enemata, administration of	57
Enemata, quantity of	57
Enemata, use of, in constipation	54
Enemata, use of, in intestinal obstruction	54
Ergot, use of, in insomnia..	18
Exercise, use of, in insomnia	19
Fæcal retention, extreme case of	50
Fæcal retention, Dr. Matthews Duncan on	46
Fæcal retention, signs of	49
Floating kidney	84
Floating kidney, case of, verified after death	88
Floating kidney, causation of	90
Floating kidney, definition of	85
Floating kidney, diagnosis of	86
Floating kidney, established by post-mortem examinations	84
Floating kidney, frequency of, in women	91
Floating kidney, prognosis in	95
Floating kidney, Sir William Jenner on	85
Floating kidney, Sir William Roberts on... ..	85
Floating kidney, statistics of	86
Floating kidney, subjective symptoms of... ..	93
Floating kidney, traumatic origin of	91
Floating kidney, treatment of	94
Gouty insomnia	14
Gouty insomnia, colchicum in	21

	PAGE
Habitual constipation	47
Habitual constipation, aloes in	48
Habitual constipation, cascara sagrada in	49
Habitual constipation, Dr. Bell on the treatment of ...	49
Habitual constipation, Dr. Kent Spender on the treatment of	49
Habitual constipation, Sir Robert Christison on the treatment of	48
Habitual constipation, treatment of	47
Holiday, use of, in insomnia	17
Hutchinson, Mr. Jonathan, on intestinal obstruction ...	62
Hypnotic use of alcohol	18
Inhalations, in phthisical laryngitis	34
Inference, therapeutic	99
Inference, therapeutic, difficulties of	99
Inference, therapeutic, in practice	101
Insomnia, affusion of water in	19
Insomnia, alcohol as a cause of	13
Insomnia, alcohol as a remedy in	18
Insomnia, bromides in	18
Insomnia, causes and cure of	1
Insomnia, chloral in	16
Insomnia, coffee as a cause of	14
Insomnia, colchicum in	21
Insomnia, deafness in	9
Insomnia, digitalis in	18
Insomnia, ergot in	18
Insomnia, exercise in	19
Insomnia, gouty	14
Insomnia, holiday in	17
Insomnia, iron in	18
Insomnia, mental shock as a cause of	5

	PAGE
Insomnia, mental strain as a cause of	5
Insomnia, mineral waters in	21
Insomnia, muscular twitching in	10
Insomnia, opium in	16
Insomnia, psychic	5
Insomnia, purgatives in	21
Insomnia, senile	15
Insomnia, shock as a cause of	5
Insomnia, soporifics in	16
Insomnia, state of brain in	4
Insomnia, subjective results of	9
Insomnia, symptoms of	8
Insomnia, tea as a cause of	14
Insomnia, treatment of	16
Insomnia, tobacco as a cause of	12
Insomnia, toxic	11
Insomnia, varieties of	3
Insomnia, water, affusion of, in	19
Insomnia, waters, mineral, in	21
Intestinal obstruction	44
Intestinal obstruction, Dr. Brinton on	61
Intestinal obstruction, Dr. Leichtenstern on	60
Intestinal obstruction, O'Beirne's tube in	56
Intestinal obstruction, Mr. Jonathan Hutchinson on	62
Intestinal obstruction, treatment of	54
Intestinal occlusion	58
Intestinal occlusion, causes of	58
Intestinal occlusion, laparotomy in	67
Intestinal occlusion, laparo-ileotomy in	70
Intestinal occlusion, operations in	67
Intestinal occlusion, recovery in	65
Intestinal occlusion, signs of	63
Intestinal occlusion, spontaneous recovery in	65
Intestinal occlusion, surgical procedures in	67

	PAGE
Intestinal occlusion, treatment of	66
Intestinal occlusion, varieties of	58
Intestinal stricture	61
Intestinal stricture, Dr. Bristowe on	62
Iron, in habitual constipation	46
Iron, in insomnia	18
Jenner, Sir William, on floating kidney	85
Kidney, floating (see floating kidney)	84
Kidneys, relative anatomy of	92
Laparotomy... ..	67
Laparotomy, indications for, in intestinal occlusion	68
Laparo-ileotomy, comparative safety of	70
Laparo-ileotomy, in intestinal occlusion	70
Laryngeal brushings	33
Laryngitis, phthisical (see phthisical laryngitis)... ..	22
Laryngoscope, use of, in phthisical laryngitis	23
Leichtenstern, Dr., on intestinal obstruction	60
Lozenges, use of, in phthisical laryngitis	35
Mental shock as a cause of insomnia	5
Mental strain as a cause of insomnia	5
Mineral waters in insomnia	21
Muscular twitchings in insomnia	10
Neglect of therapeutic teaching	102
Nervous temperament	8
O'Beirne, on defæcation	56
O'Beirne's tube	56
Opium, in insomnia	16

	PAGE
Phthisical laryngitis	22
Phthisical laryngitis, codeia in	35
Phthisical laryngitis, cough in	36
Phthisical laryngitis, counter-irritation in	32
Phthisical laryngitis, definition of... ..	22
Phthisical laryngitis, deglutition in	28
Phthisical laryngitis, diagnosis of... ..	25
Phthisical laryngitis, dyspnœa in	28
Phthisical laryngitis, frequency of, in men	25
Phthisical laryngitis, general treatment in	31
Phthisical laryngitis, hoarseness in	27
Phthisical laryngitis, inhalations in	34
Phthisical laryngitis, laryngeal brushings in	33
Phthisical laryngitis, laryngoscope in	23
Phthisical laryngitis, lozenges in	35
Phthisical laryngitis, morphia in	35
Phthisical laryngitis, opium in	35
Phthisical laryngitis, pain in	29
Phthisical laryngitis, pathology of	24
Phthisical laryngitis, prognosis in... ..	27
Phthisical laryngitis, relation of, to pulmonary consump- tion	25
Phthisical laryngitis, respiratory difficulty in	28
Phthisical laryngitis, rest in	36
Phthisical laryngitis, sputum in	30
Phthisical laryngitis, stages of	25
Phthisical laryngitis, treatment of	31
Phthisical laryngitis, voice in	27
Phthisis, chloride of calcium in	38
Phthisis, generic significance of term	38
Phthisis, medicinal treatment of	38
Phthisis, remedies in	39
Physiological research in therapeutics	112
Practice and science, relations of	106

	PAGE
Psychic insomnia	5
Pulmonary second sound, accentuation of	70
Pulmonary second sound, accentuation of, clinical import of	74
Pulmonary second sound, accentuation of, prognosis in...	79
Pulmonary second sound, accentuation of, therapeutics of	82
Purgatives, in habitual constipation	47
Purgatives, in insomnia	21
Respiratory difficulty in phthisical laryngitis	28
Respiratory symptoms in phthisical laryngitis	28
Retention, fæcal, case of	50
Retention, fæcal, Dr. Matthews Duncan on	46
Retention, fæcal, signs of	49
Roberts, Sir William, on floating kidney...	85
Sanders, Dr. James, on chloride of calcium	41
Science and practice, relations of	106
Second sound, pulmonary, accentuation of	70
Second sound, pulmonary, accentuation of, clinical import of	70
Second sound, pulmonary, accentuation of, Dr. Walshe on	74
Second sound, pulmonary, accentuation of, prognosis in...	79
Second sound, pulmonary, accentuation of, therapeutics of	82
Senile insomnia	15
Sleep, state of brain in	3
Sleeplessness, state of brain in	4
Soporifics, use of in insomnia	16
Spender, Dr. Kent, on the treatment of habitual con- stipation	49
Spontaneous recovery in intestinal occlusion	65
Sputum in phthisical laryngitis	30
Statistics of floating kidney	86
Stricture of intestine	61

	PAGE
Stricture of intestine, Dr. Bristowe on	62
Surgical procedures in intestinal occlusion	67
Symptoms of insomnia	8
Tea, as a cause of insomnia	14
Teaching, therapeutic, neglect of	102
Temperament, importance of	7
Temperament, nervous	8
Theories, false, danger of, in therapeutics	104
Therapeutic inference, difficulties of	99
Therapeutic progress, conditions of	111
Therapeutic progress, directions of	115
Therapeutic progress, recent	108
Therapeutic teaching, neglect of	102
Therapeutic triumphs	101
Therapeutics, backwardness of	98
Therapeutics, physiological research in	112
Therapeutics, scope of	109
Therapeutics, Sir Thomas Watson on	98
Tobacco, as a cause of insomnia	12
Toxic insomnia	11
Treatment of constipation	44
Treatment of constipation by enemata	54
Treatment of consumption... ..	38
Treatment of habitual constipation	47
Treatment of insomnia	16
Treatment of intestinal obstruction	44
Treatment of intestinal occlusion	66
Treatment of phthisical laryngitis	31
Tube, O'Beirne's	56
Walshe, Dr., on accentuation of the pulmonary second sound	74
Water, affusion of, in insomnia	19
Watson, Sir Thomas, on therapeutics	98



